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Astronomy Faces the War*

By OTTO STRUVE

Astronomy, unlike physics, is not in the midst of the war-research effort. When this war is won astronomers will not be able to say that their knowledge and technical experience turned the tide. But as the representatives of one of the sciences devoted to the study of matter and force in the universe, they are on the sidelines of the heroic effort of the physicists. Many astronomers are fairly good practical physicists and most of these have been called to take an active part in the war work. Secret projects in optics for the armed forces are being conducted behind closed doors at our observatories. Hundreds of astronomers render a great service by the teaching of navigation. Still others, less experienced in the design or operation of physical instruments, are, nevertheless, contributing immeasurably to our conquest of the forces of nature. These men, though they can claim no significant part in the winning of this war, find an inspiration in the realization that by their efforts science will either avert another war, or insure its victory for the free nations—just as the efforts of their predecessors—those men who developed the special optical systems now used in aerial photography, or those who gave us an understanding of the structure of the elements from their ionization and excitation in different astronomical sources, have played their part in preparing us for victory now.

Throughout the first world war astronomy remained an international science. In spite of the restrictions of censorship and the difficulties of postal communications there always remained an exchange of information between astronomers. Science then seemed to be above war. But the totalitarian governments have altered this. To them science long ago became a tool of warfare and of political machinery. Was it an accident that Japan has not contributed appreciably to the development of a pure science like astronomy in the past twenty years, or was it a well-planned official policy to spend no money on pure research the fruits of which would become available through the publications of the democratic nations? We are compelled, I think, to review our fundamental assumptions with regard to international science. So long as there are in the world strong totalitarian governments we must concentrate, in self-defense rather than by choice, upon the organization and development of *national* science. Perhaps some day we shall again be able to cherish the international character of astronomy and feel free of

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the fetters imposed by irresponsible and ignorant dictators.

The war has placed a large number of new problems before us, and has intensified many old ones. I want to review these problems and outline how the organization with which I am connected has attempted to cope with them.

My fundamental premise is that pure science is important even in total war. In this connection I can do no better than to quote the words written by Professor George E. Hale at the outbreak of the first world war:[1]

> "There remain to be considered the great majority of astronmers, whose duty and privilege it will be to carry forward their investigations during the period of the war. Recent events, both in Europe and the United States, have emphasized in the strongest manner the dignity and importance of scientific research. The tendency shown by some critics of limited view to measure the importance of research in terms of its immediate applicability to practical affairs is not justified by the facts. On the contrary, it is certainly fair to say that no science has done more for the world than astronomy. This is not chiefly because of its determination of the time, or the system of navigation it has developed, fundamentally important though these be. It is rather because of the sweeping change in human thought that astronomy has wrought and is still effecting, and the stimulating influence of the oldest of the sciences upon other sciences of later development. But I need not dwell on a theme which has been so luminously developed by Poincaré in 'La Valeur de la Science.' In his demonstration of the importance of astronomy to the world we find ample justification for the work we are doing, and the strongest encouragement to continue its pursuit in the interest of national progress."

1. *We must make American astronomy self-sufficient.* By this I mean that we must develop all branches of our science, and not rely upon astronomers in other countries to supply important links in the continuous chain of progress. Until a few years ago astronomy in this country suffered from two serious weaknesses: It did not possess a sufficiently large number of theoretical workers in astrophysics and it did not provide in a sufficient number of universities the kind of training which would tend to eradicate this lack of theoretical knowledge. This was especially serious because in dynamical astronomy we did have some of the greatest research workers as well as teachers. I remember that when I came to the Yerkes Observatory in 1921 the training of graduate students consisted wholly of practical work in observing, measuring, and reducing. No courses in theoretical subjects were offered, and no knowledge of theoretical physics, the quantum theory,

[1] *Publications of the American Astronomical Society*, **3**, 315, 1917.

thermodynamics, etc., was expected for the degree of Ph.D. I believe that with the one notable exception of Princeton, the graduates of most of our astrophysical departments were seriously lacking in theoretical training. If this statement may appear to you to be exaggerated, I call your attention to the amazing fact that for a period of some twelve years—from 1920 to 1932—the *Astrophysical Journal* contained only about one theoretical paper for every twenty papers on observational topics. During the same period the *Monthly Notices of the Royal Astronomical Society* had approximately half of their titles—and much more than half of their pages—devoted to theoretical subjects.

The weaknesses which I have mentioned became apparent many years ago. Our superb development in observational and dynamical astronomy threatened to bog down because we did not possess enough theoretical astrophysicists to interpret our observations and to derive from them a better understanding of the laws of the universe. This situation has been party remedied through the concerted efforts of several universities, and I am glad to say that the Yerkes Observatory and the University of Chicago have had an important part in this development. We recognized long ago that it was inefficient to rely upon astronomers of other countries to evaluate our observations in terms of theories, and we set out to remedy this weakness by appointing to our staff the most capable theoretical workers in astrophysics whom we could find. Political unrest in Europe and elsewhere had driven some of them into exile. Others came because they were attracted by the splendid observational facilities we could offer them. The wisdom of this policy has now been amply demonstrated. It was essentially national in character; it has helped to make this country independent in its scientific development, and it has given us this independence in the cheapest and fastest manner.

The question arises whether these efforts are sufficient. I think they are not. In the first place there are still some branches of astronomy without proper representation in this country. I am not competent to discuss in detail the situation in regard to the nautical almanacs, but I am rather seriously worried over the fact that positional work on asteroids, in which Professor Van Biesbroeck at Yerkes is taking an active part, depends for its success almost wholly upon the computations of orbits and ephemerides at the Berlin *Recheninstitut*. We have photostats of proofs of such ephemerides for a part of the current year, but we are hardly prepared to continue this type of work unless the computational data are supplied from Germany.

In astrophysics I feel our greatest lack at the present moment is in men who do the work of the preliminary digesting of the observations, in preparation for the pure theoreticians. We unquestionably have in this country the best observers and the best theoreticians. We still lack a sufficient number of observers who are trained in theory and who fit

their observations to the specific requirements of the theoreticians. The situation is not dangerous, provided we recognize it and train young men in the required type of work. This will remedy what is now a detachment between theory and observation, leading to excessive speculation on one side and to waste of telescope observing time on the other.

2. *We must increase the efficiency of our observational effort.* The war effort has temporarily diminished facilities for the construction of new astronomical instruments. Fortunately, we possess in America the largest, as well as the best, telescopes of all varieties; but their distribution is exceedingly uneven. It is no exaggeration to say that more than 90 per cent of all observational work in astrophysics comes from four or five large observatories. Dozens of other observatories are equipped with good telescopes. A few produce results of outstanding value. One or two hold the enviable record of providing more knowledge per dollar of investment than any other observatory. But the majority of the smaller astronomy departments are inefficient. Although equipped with instruments intended for research they have at best become good student observatories. At their worst, the instruments are not even used properly for teaching. The reason for this situation is the fact that limitation of scope almost always produces a decline in the quality of research. If I can only determine the magnitude and color of a star with my equipment, but cannot examine its spectrum, I am likely to forget gradually that spectra exist. If I find that the colleague who has access to a spectrograph is able to obtain in a few minutes or hours of observing more significant information about the star than I can obtain after years of effort, I am ready to feel despondent and abandon even those efforts where the disadvantage of my instrument is less noticeable. A very restricted observatory equipment usually works successfully if it is directed by a person who has been trained in a large institution or by teachers whose scope of knowledge is wide and encompasses many different branches of astronomy. But the limitation often results in a gradual decline from the lack of competent successors.

The unevenness of present-day distribution of astronomical equipmet must be remedied. If we fail to do something about it we shall sterilize our large observatories and cut them off from the sources of new human material. The largest and best of all our observatories does not regularly train new astronomers. The few remaining large observatories cannot prosper if they cut off the influx of well-trained men from the hundreds of colleges and universities.

The situation can be remedied if we are willing to continue to extend cooperation among the astronomical departments of the country. We of the Universities of Chicago, Texas, and Indiana were first in adopting a plan of cooperation for the use of the equipment of the McDonald Observatory. Our astronomers have now for several years had the privilege of using the world's second largest telescope. The results

have been successful beyond all expectations. We have carried out research in many different fields and have not only added to human knowledge in proportion, let us say, to the man-aperture-hours at the telescope, but have immeasurably strengthened American science by attracting such brilliant foreign astronomers as G. P. Kuiper and P. Swings. If a few specific accomplishments are desired for illustration, I might mention Edmondson's measurements at the University of Indiana of radial motions of stars as faint as the 12th or 13th magnitude; Kuiper's discovery of a new brand of stars—the sub-dwarfs—all characterized by very large space motions, and all about three to five magnitudes fainter in the Hertzsprung-Russell diagram than the main sequence; Greenstein's study of a star of exceptionally small atmospheric hydrogen content; Swings' and Elvey's discovery of two new molecules, *OH* and *NH*, in comets; Keenan's work on stars characterized by exceptionally strong bands of *CH*; and my own recognition of large interstellar masses of glowing hydrogen.

I recommend to all astronomers and university administrators that our plan of departmental cooperation be carefully studied to serve as a basis for similar plans in other institutions. We are also eager to extend our own plan, though we recognize that with our *existing* equipment on Mount Locke we have almost reached the useful limit of cooperation.

3. *We must institute national planning in astronomy,* to take the place of the unsystematic choice of problems which has dominated our science in the past. Critics of the procedure of planning usually deplore the suppression of individual initiative. I am certain that this criticism is unfounded. Not only does national planning not suppress the initiative of competent investigators, but it gives the greatest possible scope to the utilization of private initiative.

Perhaps it will be instructive to review briefly our present system of forming an observatory research program. I have, of course, in mind my own institution. Some 20 or 25 years ago the work of a stellar spectroscopist fell in two classes. The one consisted in the participation in a general spectrographic program of radial-velocity determinations of about 1000 bright stars of types O, B, and A. The program was originally intended to extend the brilliant research on radial velocities by Frost, Adams, and Kapteyn, which, among other things, had led to the discovery of the K effect and of the relatively slow motions of the B stars.

But, as often happens, the enlarged program which absorbed perhaps 20 years of telescope time, yielded little new information that was not immediately surpassed by results from other observatories. The work was clearly badly planned and badly executed. I have sometimes wondered whether astronomy would have lost perceptibly if this program had never been started. I believe it did give Oort material for his original

work on galactic rotation, but the observations by Plaskett and Pearce at Victoria and by Campbell and Moore at the Lick Observatory gave far better results only a few years later. There was an indirect importance to the Yerkes observations: they provided the background for all the later work on the physical interpretation of spectra of early type and led to the discovery of Stark effect, of stellar rotation, of turbulence in stellar atmospheres and of dilution effects in extended shells. Yet, throughout the duration of the original program it was uninspired and sterile. The other part of the work, more interesting and fruitful, consisted of several small research problems. The choice of these problems was somewhat casual. If an easy spectroscopic binary was available that binary was investigated not so much because astronomy required the knowledge of its orbit, but because it was easily accessible with our telescope and promised quick results. I am aware from my association with other astronomers that this opportunist method of choice of problems still persists. Even where directors of observatories are taking a firm hand in the organization of the work the planning, if there is any, is more nearly on an institutional scale than on a national scale.

The *International Astronomical Union* through its many committees attempted to guide astronomers in the choice of problems, but the efforts of these committees were relatively insignificant. Moreover, they were often thwarted by the extreme diversity of interests, training and prejudices among the various national groups. We must hope that after the war the Union will resume its activities and provide a measure of *international* planning. Independently of it we need a strong central organization for *national* planning. The astronomy section of the *National Academy of Sciences,* appropriately enlarged by inclusion of competent men from all branches, could well undertake the organization of a planning bureau. I suspect that the idea will meet with some disapproval on the part of (1) those who genuinely fear bureaucratic interference with individualistic effort and, (2) those who prefer to be left undisturbed in the pursuit of easy, though perhaps not very useful enterprises. I believe that both groups may be reassured: a planning bureau as here suggested would have no power to impose its directions upon anyone. Its purpose should be to stimulate the most competent astronomers in the country to review from time to time the requirements of the entire science and to make the results of their findings known to all of their associates and students. In an unorganized way this is already being done in various symposia, summarizing articles, observatory reports, and society meetings. The disadvantage of the present system is that it more nearly resembles the activities of irresponsible pressure groups intent upon an all-out effort in this or in that particular field of study, than the work of an organization which bears a clearly defined responsibility toward the entire nation.

4. ***We must plan for continued means of publication of important re-***

search. Heretofore the publication of astronomical research in America was divided between the regular observatory publications and the technical journals—the *Astronomical Journal, Astrophysical Journal,* and the *Publications of the Astronomical Society of the Pacific.* The tendency, I think, has been in recent years for newly established observatories to rely more upon the journals and less upon observatory publications. Only some of the largest and wealthiest universities are able to distribute their publications to other observatories free of charge. So long as these universities are able to continue this practice it will be welcomed by the recipients of their generosity. But it has been my experience at the University of Chicago that administrators and University editors look askance upon the strange tradition among observatories to exchange their publications free of charge. I suspect that other observatory directors have had similar experiences. The result is a growing disproportion among items received and items mailed out. Sooner or later, the whole question of observatory publications will have to be reviewed.

In the meantime our technical journals—I speak, of course, only for the *Astrophysical Journal*—are in very grave danger. They have never been self-supporting. In the case of the *Ap. J.,* the University of Chicago meets an annual deficit of roughly $1,000.00. The number of foreign subscriptions has dwindled to perhaps 10 per cent, or even less, of the pre-war normal. Domestic subscriptions have slightly increased, especially since the *Journal* became affiliated with the American Astronomical Society. At the present time we are able to operate fairly satisfactorily, because five of the country's largest observatories, Mount Wilson, Harvard, Lick, Yerkes, and McDonald, publish a large part of their results in the *Journal* and pay for excess charges over $200 of composition in each volume.

If conditions should become worse we can start planographing the entire journal—we already use the offset method of reproduction for some of our long tables. There is all reason to believe that we shall be able to maintain our means of publication, but it is imperative that individual subscriptions by members of the society take the place of a part of the lost foreign subscriptions.

There is another aspect to the problem of publications which is even more pressing. I refer to the fact that during the past 20 years, or more, we have been almost wholly dependent upon other countries for advanced textbooks, monographs, and encyclopedic works on astronomy and related subject. In spite of the almost unbelievable price of $250.00 for the "Handbuch de Astrophysik" and in spite of the fact that most of the actual research had been made in this country and in England, no modern compendium on astrophysics has been even attempted in this country. Similarly, there have been only one or two advanced textbooks on astrophysics in America since Frost's revision of Scheiner's famous "Spectralanalyse der Gestirne." To insure the success of our teaching we

must have textbooks like Unsöld's "Physik der Sternatmosphären" and like the cooperative volume on astronomy and astrophysics which appeared some years ago in Russia under the editorship of the late B. P. Gerasimovič. We have some very fine elementary textbooks, culminating, of course, in the famous two volumes of Russell-Dugan-Stewart. But we need one or two more advanced books. The "Astrophysical Monographs" sponsored by the *Astrophysical Journal* are intended to overcome this lack. We have already printed two advanced textbooks by Chandrasekhar, one on stellar structure, the other on the dynamics of stellar systems. We have also printed several volumes on special topics, such as Russell and Moore's "The Masses of the Stars," Merrill's "The Spectra of Long Period Variables," and Bok's "The Distribution of the Stars in Space." The outlook, however, is not bright; there is not nearly enough money available to produce anything like the German "Handbuch," and I am afraid there are few astrophysicists who could do as fine a job as Unsöld did in writing his textbook.

The field of serious popularization has been recently promoted through the publication of the "Harvard Books on Astronomy." They are more than sufficient to satisfy the needs of amateurs and even of professional astronomers for non-technical presentations of various subjects.

We should probably develop and strengthen a specific type of popular literature to combat the ever-growing evil of astrology. But I have no personal knowledge of this important subject and must refer to the statements by Dr. Bok's anti-astrology committee of the Association of Scientific Workers.

5. *We must preserve efficient staffs in the observatories and teaching departments.* I have already stated that a number of our most valuable faculty members have left in order to do war work. In the present emergency of the country it is, of course, necessary to encourage such individual participation. However, it would help in preserving the highly specialized personnel which we have built up, if government research planning bodies would assign more problems to existing departments. We have had a few calls of this sort, and they have not only met with enthusiastic response on the part of the astronomers, but have materially assisted in preserving the morale of men who sometimes feel that is it almost useless to continue pure research not connected with the war.

YERKES OBSERVATORY, WILLIAMS BAY, WISCONSIN.

On the Brightness of Comets

By N. T. BOBROVNIKOFF

Introduction

The problem of the brightness of comets has two aspects. We may try to determine the law of the variation in the brightness of comets depending on their heliocentric and geocentric distances by using already existing observations. On the other hand, we may compute their brightness on the basis of information on the physical properties of the substance shown to exist in the heads of comets and compare it with the actually observed brightness.

In my opinion, the second approach is not worth attempting until we have settled the first problem, namely, how the comets vary in brightness. Heretofore the problem has not been successfully solved with the resulting confusion in the formulae adopted for the calculation of the future brightness of newly discovered comets.

The author has just completed an investigation[1] which bears on this problem. The results of this investigation are presented here without the details for individual comets for which the reader, if interested, must consult the original paper.

It has been shown in the first part of this work[2] that visual estimates of the total brightness of comets are affected by large systematic errors making it impossible to accept these estimates at their face value. Systematic errors distort the shape of the light curves of comets to such an extent that the conclusions derived from them may be wholly fictitious.

A method has been developed by means of which the observations of the brightness of forty-five comets, for which enough observations were available, were reduced to a homogeneous photometric system common to all of them, namely, the International Photovisual system, and to the average aperture of the telescope, 2.67 inches. Normal places for certain mean dates were then formed, and the probable errors of these normal places are considered as indices of the precision of observation.

The problem to be treated in this part of the investigation is to determine the law of the variation of the brightness of comets that would not be at variance with the observational facts about comets and would be common to all comets considered.

The light curve should represent the normal places with a precision comparable with the precision of observations. Considering the average probable error of one normal place, $\pm 0^{M}.089$, and the existence of comets which change their brightness abruptly in a very short interval of time, it is perhaps too optimistic to expect that the brightness of a comet can be expressed by any formula at all, least of all by a formula common to all comets. It is shown, however, in this investigation that

the problem is solved in a satisfactory way by dealing with two quantities called photometric parameters which are determined from the set of equations based on normal places for the observed brightness of the comet. The probable error of one observation derived from the least squares solution is then

$$R_o = \pm 0.675 \sqrt{[vv]/(N-2)},$$

and this should be compared with the average probable error, $\overline{R}_1$, of normal places for any particular comet.[3] This latter quantity can be expressed as follows:

$$\overline{R}_1 = \pm 0.675 \ (1/N_2) \ \Sigma \sqrt{[vv]/N_1 (N_1-1)} \qquad (2)$$

where N_1 is the number of individual observations entering into a normal place, N_2 is the number of normal places for the comet, and v is the departure between the observed and computed values of cometary magnitudes.

It is proper here to mention the extensive work by Holetschek[4] which deals partly with our problem. Holetschek gathered an immense amount of material concerning the physical observations of comets up to the year 1835. In this respect his work needs few improvements, if any. He investigated only a few comets which appeared after 1835, and only the periodic ones in any detail. He based his work on the assumption that the brightness of comets cannot be represented by any formula at all. He computed then the "reduced" brightness of comets on the basis of the formula for the reflection of light and compared his computations with the observations.

The investigations by Russian writers, especially by Vsessviatsky, published mostly in the *Russian Astronomical Journal*, approach the present paper in their point of view. The main criticism of these writers is that they all used observations as given without reducing them to any particular photometric system. This flaw in the method was realized by Vsessviatsky who tried to evaluate the systematic differences between various observers[5] but gave up the attempt as fruitless.[6] The starting point of the present investigation was the reduction of all available observations to one photometric system which could be accomplished in a satisfactory way.

Photometric Formulae

It was realized a long time ago[7] that the formula for the simple reflection of light,

$$J = J_0/\Delta^2 r^2, \qquad (3)$$

where J is the observed brightness, J_0 is the brightness at unit distance from the sun and the earth, Δ is the geocentric distance, and r is the heliocentric distance, is not applicable to comets and that the exponent of r should be in general greater than 2.

Considering the extreme complexity of physical processes involved in the formation of the comet's atmosphere, we should not expect any very simple formula applicable to all cases. In general we may put

$$J = [J_o/\Delta^2 f(r)]\ \phi\ (r, \Delta) \tag{4}$$

where $f(r)$ is a certain function of the heliocentric distance and $\phi\ (r,\Delta)$ is a function of both r and Δ, such as the phase-angle. It will be shown later that there is no evidence whatsoever that the light of the comet depends on the phase-angle so that $\phi(r,\Delta)$ may be considered equal to unity.

Converting the above formula into stellar magnitudes we have

$$H = H_o + 5 \log \Delta + 2.5 \log f(r). \tag{5}$$

Since log Δ is a known quantity we may put

$$H - 5 \log \Delta = H_\Delta \tag{6}$$

which represents the observed brightness of the comet reduced to the geocentric distance $\Delta = 1$. We have then

$$H_\Delta = H_o + 2.5 \log f(r). \tag{7}$$

Now it is the record of experience that when H_Δ is plotted against log r the result is a nearly straight line, and we may denote this fact by putting

$$\log f(r) = n \log r. \tag{8}$$

The final photometric formula in stellar magnitudes is then

$$H_\Delta = H_o + 2.5\, n \log r, \tag{9}$$

or in the intensities of light

$$J = J_o/\Delta^2 r^n. \tag{10}$$

If we have a sufficient number of observations the photometric parameters H_o and n can be determined by the method of least squares. Comets are not solid bodies, and their brightness should depend on the amount of gaseous and solid material liberated from the nucleus. This in turn should depend on the heliocentric distance; the nearer the comet to the sun, the greater is the production of particles and molecules contributing to the total light of the comet. However, when one recalls comets like 1886 V (Brooks) which with a small perihelion distance 0.3 never had a trace of a tail and, on the other hand, the great comet of 1811 with $q = 1.0$ and a tremendous development of the tail, one is forced to the conclusion that the heliocentric distance is not the only factor controlling the formation of cometary atmospheres. The spectra of comets and polarization measurements also show a large variation in the proportion of reflected to intrinsic light. There is therefore no reason why the exponent n in the above formula should be the same for all comets or, indeed, for the same comet if the comet is observed a sufficiently long time. We should nevertheless hope that a statistical study would reveal the general features of the variation in the brightness of comets. Since the present study is based on 4,447 observations a statistical approach to the problem is indicated.

We must make one assumption to begin with, namely, that comets differ from each other in intrinsic brightness. Therefore, in order to compare different comets, we must adjust in some way their light curves.

This can be done in the simplest way by making use of the photometric parameters H_o (the so-called absolute brightness) derived for each comet separately. This represents the average brightness of the comet referred to the unit distance from the sun and the earth. In the diagram H_Δ against log r the straight line (or its prolongation) derived from the least squares solution intersects the ordinate log $r = 0$ at a point corresponding to H_o. This intersection is of course different for different comets. If we now shift the whole line parallel to itself along the ordinate until the straight lines of different comets intersect the log $r = 0$ ordinate at the same point, say $6^M.00$, we can combine the data for individual comets for a statistical study. It should be understood that this does not involve any assumption as to the shape of the light curve. If in the diagram H_Δ against log r the observed points are situated on a curve rather than on a straight line, they remain so situated no matter how far we move the curve up or down along the H_Δ axis.

Adopting then $H_o = 6^M.00$ as the magnitude to which all comets are to be referred, we have two quantities

$$x = \log r$$
$$y = H_\Delta - H_o + 6^M.00$$

to be used for a statistical analysis.

The plot of y against x (Figure 1) shows that the correlation is linear. Since the behavior of comets after perihelion may be different as compared with the behavior before perihelion it is necessary to make a double solution.

The coefficients of linear correlation are very high for both cases. We have:

before perihelion $\rho = 0.929 \pm 0.005$
after perihelion $\rho = 0.901 \pm 0.007$.

The test of linearity

$$N(\eta^2 - \rho^2) < 11.37$$

is satisfied for both coordinates. The lines of regression are

before perihelion $Y = 8.488\,X$
after perihelion $Y = 8.034\,X$.

These figures correspond to the exponent $n = 3.39$ before perihelion and 3.21 after perihelion in the photometric formula (10).

The two solutions are so similar that there is no reason to treat the observations before and after perihelion separately. There seems to be no systematic difference in the behavior of comets before and after perihelion so far as their light is concerned. Combining all observations we find that the criterion of linearity is satisfied, and the coefficient of correlation is very high ($\rho = 0.914 \pm 0.004$). The equation of the line of regression is

$$Y = 8.222\,X$$

corresponding to the exponent $n = 3.29$.

It is evident that the simple photometric law

$$J = J_0/\Delta^2 r^n \qquad (10)$$

is applicable to comets in the statistical sense, although some individual comets may greatly depart from it.

The variation in the visual magnitude of comets considered here was from $-1^M.1$ (comet 1861 II) to $14^M.0$ (comet 1910 II), a range of 15 magnitudes. The variation in the radius vector was from 0.38 (comet 1903 IV) to 3.90 (comet 1904 I). The results obtained in this investigation seem to be applicable to the great majority of comets. There are, however, observational limitations which should be taken into account. As is seen from Figure 1, the majority of observations are concentrated between the values of -0.10 and $+0.10$ for log r. They correspond generally to moderate magnitudes, say, between 3^M and 8^M. For these magnitudes the scattering is very small. On either side the scattering increases very considerably, corresponding to very low and very high magnitudes. Whether this is due to the uncertainty of the photometric scales and the difficulty in estimating the magnitudes of comets or to some inherent properties of comets at very large and very small heliocentric distances is difficult to say.

It remains now to consider other photometric formulae proposed from time to time. They are all based on the behavior of some exceptional comets and certainly are not applicable in a general way.

Long ago there was much discussion as to whether comets shone only by the reflected light of the sun or were self-luminous bodies. Newton was of the former opinion; Schroeter and W. Herschel adhered to the other.[8] In the latter case the brightness of the comet would be independent of the radius vector and represented by the formula

$$J = J_0/\Delta^2. \qquad (11)$$

The brightness of some comets with a small variation in r, a small exponent n, and a large variation in Δ can indeed be represented by the above formula. In the present study such is comet 1937 f, with $n = 0.7$. Needless to say, in the great majority of cases the brightness depends strongly on the radius vector.

Deichmüller,[9] on the other hand, arguing that the quantity really observed is the surface brightness rather than the integrated magnitude, proposed the formula

$$J = J_0/r^2. \qquad (12)$$

This formula was at once challenged.[10] In most cases the closest approach of the comet to the earth is near perihelion time so that the photometric curve has but one well-expressed maximum. However, in cases when the two dates are separated by many days, as in comets 1903 III and 1903 IV, the two maxima of brightness, one due to the approach to

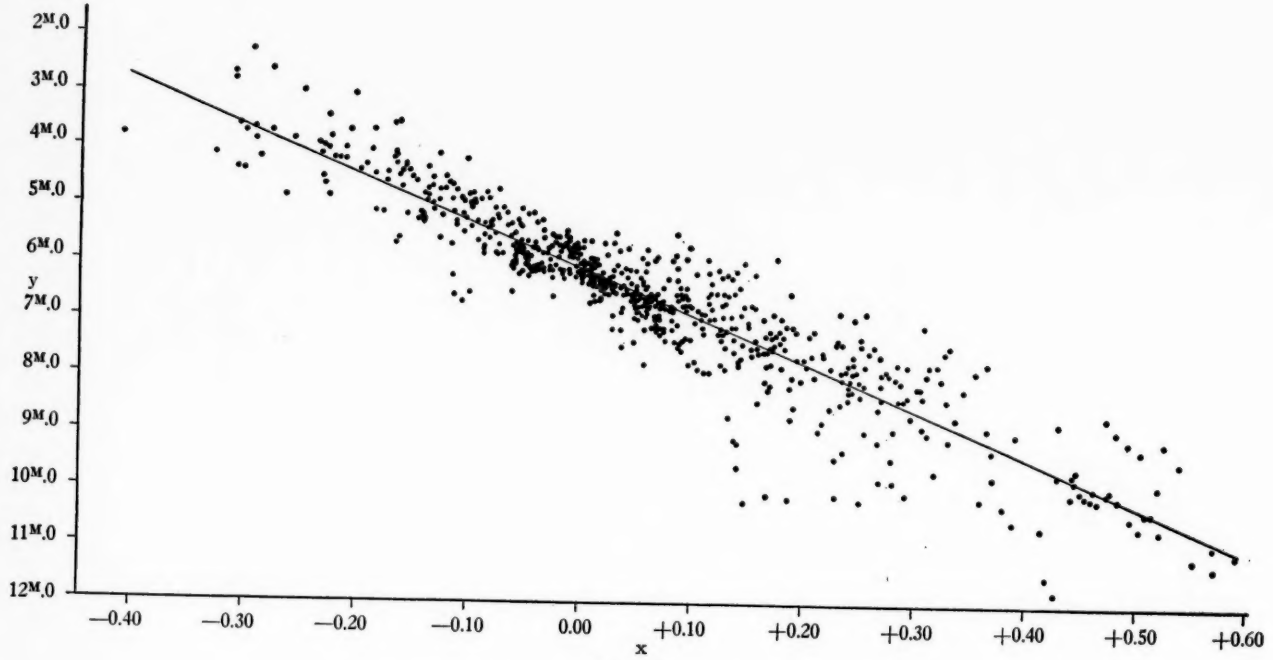

FIGURE 1
Correlation between $x = \log r$ and $y = H_A - H_O + 6^M.00$.

the sun and the other to the approach to the earth, are very distinctly marked.

More recently Filippov[11] proposed the formula

$$H = H_0 + 5 \log \Delta + (ar + b) \log r \qquad (13)$$

in which before perihelion $a = 9$ and $b = 0$. This formula is based on the supposed variation of the exponent n which was found to decrease with the decreasing r. This is not confirmed by the results obtained in the present investigation. Since Filippov does not indicate the comets studied by him, a detailed comparison is impossible.

The same formula was suggested by Vsessviatsky.[12] Among comets studied by him two, 1914 V and 1899 I, are included in the present investigation. The first comet does not show any variation of n depending on the heliocentric distance. The other comet has a distinct break in its light curve which can be satisfactorily represented by a combination of two periodic variations. Generally speaking, such an apparent dependence of n on r can be explained by the fact that fainter magnitudes observed with more powerful instruments have been systematically made fainter in comparison with naked-eye magnitudes.

Finally Deutsch[13] proposed the formula

$$H = H_0 + 5 \log \Delta + a \log r + b (\log r)^2 \qquad (14)$$

which he established in his paper on comet 1930 III. There is some evidence for this formula, but the problem can be solved in a better way otherwise. Formula (14) implies a continuous deviation from the straight line based on formula (10), which is certainly not the case even with individual comets. Statistically, the straight line relationship between H_Δ and log r holds throughout the whole range of observations.

Having determined the photometric law applicable to the total brightness of comets, we must see whether this brightness depends in any way on the phase-angle, α, that is, the angle at the comet between the directions to the sun and to the earth. It is obvious from Figure 1 that no great dependence of the observed magnitude on the phase-angle is to be expected.

In order to settle the question definitely and to see whether any statistical relationship exists between the brightness and the phase-angle, residuals o-c in the least squares solutions for the parameters H_0 and n have been plotted against the corresponding values of α. The result is Figure 2 which does not show any systematic variation of the residuals as a function of α. The brightness of comets should be greater than the average for small α and lower for large α, and therefore we should expect negative residuals for small α and positive residuals for large values of α. As is seen from the diagram, there is little systematic trend in the residuals.

When the least squares solution for H_0 and n is carried out, it is usually found that the probable error of one observation is several times

Correlation between $x = \log r$ and $y = H_\Delta - H_0 + 6^M.00$.

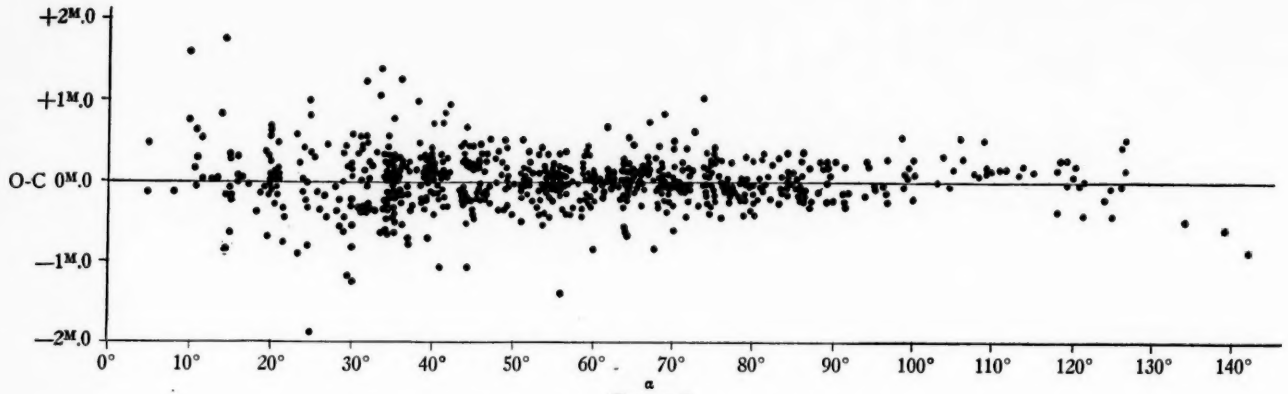

FIGURE 2
Correlation between the phase-angle, α, and the departures o-c.

larger than the average probable error of normal places. In order, therefore, to reconcile the observed and computed values some improvements must be made in the solution.

It was found that we can improve the solution for the parameters H_o and n in a comet in three different ways involving very different physical models; for some comets we must postulate a sudden break in the light curve; with different parameters for different parts of the curve; for a few comets a lag in the development improves the situation; most often, however, a periodic variation is indicated. Sometimes no choice between these solutions can be made on the basis of the residuals or probable errors. Considering all the forty-five comets, the periodic solution is to be preferred as more consistent with the general behavior of the comets. In a few cases, however, there is no other alternative but an abrupt change in the light curve.

Photometric Parameters

The results of the detailed study of the brightness of the comets are summarized in Table 1. In this table are given: the perihelion distance, q; the absolute brightness, H_o, and the exponent, n, with their probable errors; the maximum and minimum heliocentric distances, r, during the period of observation; the average probable error, R_1, of the normal places derived from a combination of individual observations; and, finally the probable error, R_e, of one normal place, obtained in the least squares solution.

TABLE 1

Summary of Results

No.	Comet	q	H_o	p.e.	n	p.e.	r max.	r min.	R_1	R_e
			M	M					M	M
1	1858 VI	0.579	3.39	0.45	3.47	0.48	0.64	0.59	0.116	0.126
1	1858 VI	0.579	4.30	0.14	4.50	0.11	0.85	0.58	0.051	0.038
2	1861 II	0.822	5.03	0.06	0.82	0.29	1.53	0.92	0.124	0.199
3	1862 III	0.963	5.31	0.02	8.88	0.41	1.08	0.96	0.048	0.058
3	1862 III	0.963	5.58	0.01	3.47	1.01	1.08	0.98	0.064	0.084
4	1874 III	0.676	6.05	0.07	4.95	0.21	1.00	0.68	0.051	0.114
5	1881 III	0.769	5.59	0.02	2.59	0.06	1.92	0.77	0.044	0.100
6	1884 I	0.766	4.64	0.15	4.19	0.32	2.14	1.21	0.169	0.162
6	1884 I	0.766	5.39	0.09	3.86	0.49	1.06	0.76	0.134	0.171
7	1886 II	0.479	6.66	0.03	2.05	0.06	1.78	0.48	0.104	0.090
8	1886 IX	0.663	4.71	0.09	2.24	0.30	1.14	0.66	0.124	0.175
9	1890 II	1.908	5.63	0.13	2.51	0.14	3.30	1.91	0.093	0.088
10	1893 II	0.675	6.40	0.10	1.76	0.35	0.97	0.68	0.127	0.117
11	1898 I	1.095	4.78	0.06	5.86	0.19	1.52	1.11	0.066	0.086
12	1899 I	0.327	6.68	0.06	3.17	0.16	1.78	1.03	0.137	0.185
13	1900 II	1.015	8.52	0.04	6.74	0.28	1.34	1.02	0.078	0.102
14	1902 III	0.401	6.77	0.02	2.63	0.06	1.68	0.58	0.085	0.081
15	1903 IV	0.330	6.50	0.03	2.38	0.06	1.51	0.37	0.113	0.109
16	1904 I	2.707	3.60	0.24	3.25	0.20	3.90	2.75	0.083	0.091
17	1906 VII	1.215	7.58	0.12	6.89	0.55	1.28	1.21	0.082	0.030
18	1907 IV	0.512	4.39	0.06	3.62	0.12	2.31	0.51	0.107	0.256
19	1908 III	0.945	3.94	0.07	5.14	0.15	1.95	0.94	0.118	0.121
20	1910 II	0.587	5.62	0.05	4.03	0.22	2.74	0.59	0.112	0.223
21	1911 II	0.685	7.42	0.05	4.14	0.25	1.16	0.71	0.119	0.130

No.	Comet	q	H_0	p.e.	n	p.e.	r max.	r min.	R_1	R_2
			M	M					M	M
22	1911 V	0.490	5.64	0.02	3.64	0.05	1.90	0.50	0.080	0.136
23	1911 VI	0.788	6.31	0.05	3.64	0.26	1.20	0.79	0.088	0.137
24	1912 II	0.716	6.08	0.03	3.14	0.09	2.04	0.72	0.110	0.160
25	1913 II	1.457	7.67	1.68	4.55	3.67	1.52	1.46	0.106	0.153
26	1913 IV	1.356	6.77	0.95	6.77	1.18	1.40	1.36	0.084	0.136
28	1914 II	1.198	10.44	0.19	0.10	0.65	1.42	1.20	0.127	0.137
29	1914 V	1.104	1.70	0.09	3.50	0.08	3.72	1.10	0.111	0.167
30	1915 II	1.000	4.51	0.28	3.96	0.21	2.99	1.38	0.086	0.200
31	1917 II	0.764	9.44	0.55	−1.77	0.73	0.93	0.76	0.136	0.159
32	1917 III	1.687	8.29	0.98	1.97	0.67	2.25	2.02	0.088	0.237
33	1919 III	0.485	10.23	0.03	5.53	0.14	1.17	0.62	0.112	0.097
34	1921 II	1.009	6.94	0.24	5.53	0.75	1.18	1.01	0.091	0.129
35	1925 II	1.109	5.89	0.07	3.07	0.18	1.78	1.16	0.091	0.080
36	1930 II	0.672	8.34	0.12	4.27	0.38	0.92	0.67	0.109	0.119
37	1930 III	0.483	8.64	0.03	4.50	0.07	1.45	0.48	0.087	0.111
38	1932 V	1.037	7.36	0.07	11.40	0.75	1.18	1.04	0.058	0.133
39	1932 VI	2.313	5.03	0.49	2.46	0.39	3.46	2.73	0.054	0.082
40	1932 X	1.131	8.88	0.12	2.09	0.54	1.40	1.13	0.097	0.134
41	1935 I	0.811	9.77	0.06	2.75	0.38	1.09	0.81	0.098	0.121
42	1936 a	1.100	6.72	0.05	4.75	0.28	1.33	1.10	0.066	0.079
43	1937 b	1.733	6.42	0.18	3.01	0.26	2.33	1.73	0.067	0.101
44	1937 c	0.621	10.26	0.02	3.88	0.08	1.47	0.68	0.064	0.065
45	1937 f	0.863	6.19	0.02	0.55	0.16	1.14	0.86	0.056	0.076

We notice a large variation in the value of n, from −1.77 to +11.40. Taking the weights of n to be inversely proportional to their probable errors, we obtain the weighted mean value of n

$$n = 3.32 \pm 0.16.$$

This figure is almost identical with $n = 3.29$ obtained previously by the method of linear correlation between x and y. Such close agreement of the two mean values of n shows that no systematic changes are introduced by the application of the periodic corrections. In other words, for a statistical study it is not necessary to apply the corrections due to the periodic variation in the light of comets.

It is evident that the exponent n has only a statistical significance. Any one comet can have n equal to a negative quantity (1917 II) or a very large positive quantity (1932 V), and the prediction of the brightness of a newly discovered comet is necessarily hazardous. The commonly followed practice of using $n = 4$ is certainly preferable to $n = 6$ or $n = 2$; $n = 3$ would probably be even better than $n = 4$, but conspicuous failures of comets to live up to expectations cannot be altogether avoided.

Vsessviatsky[14] obtained $\bar{n} = 4.12 \pm 0.24$ for forty-six comets. The difference in the value of $\bar{n}$ between Vsessviatsky and the present investigation must be ascribed mainly to the progressive correction of the observed values of H in order to reduce the observations to a homogeneous system. These corrections, being positive for 0^M and gradually diminishing toward $7^M.5$, would tend to make the contrast between the

maximum and minimum brightness of a comet less pronounced; that is, they would result in a smaller value for n.

It is not easy to account theoretically for the brightness of comets since the situation is very complex, but it is possible to show that the exponent n should be in general greater than 2 and very near to 3.

For three comets, 1858 VI, 1862 III, and 1884 I, it was found impossible to represent the whole light curve by one set of parameters. It is probably significant that all of these comets were poorly observed at a time when photometric scales were practically non-existent. Comet 1884 I, however, was remarkable for its variability in light and aspect. The other two comets were also outstanding in their nuclear activity. There is no doubt that some comets do show extraordinary changes in light impossible to represent by any formula. One of the most extreme examples of intrinsic variability was the periodic comet 1892 III (Holmes). The following figures are taken from Holetschek's investigation,[15] with H_0 reduced on the basis of $n = 2$:

TABLE 2

BRIGHTNESS OF COMET HOLMES

Date	H_0 M
1892 Nov. 8	2.5
Nov. 10	1.9
Nov. 13	2.6
Nov. 18	3.0
Nov. 25	3.2
1893 Jan. 11-14	11.1
Jan. 16	2.0
Feb. 10	4.1
Mar. 4	7.3
Beg. Apr.	10.4

In the apparitions of 1889 and 1906 the absolute brightness of this comet was uniformly and nearly the same, $11^M.8$. The comet has not been seen since.

A comet perhaps related in its physical behavior to Comet Holmes was another one of short period, Comet Westfal (1913 VI), which was not included in the means derived from Table 1. This comet collapsed and entirely disappeared before it reached its perihelion. The only other comet known to me which met the same fate was 1897 III (Perrine). It was observed between October 16 and November 27, being exceedingly faint at the last observation.[16] Perihelion passage was due on December 8. Comet 1926 III (Ensor), although greatly diminished in brightness, could still be observed after perihelion,[17] and therefore its behavior is not altogether the same as that of the two mentioned above. No doubt other instances of similar behavior will be revealed by careful study.

These examples indicate that perhaps some comets carry in them the seeds of their own destruction. For a time they may behave in a regular fashion only to collapse unexpectedly at any moment and with-

out warning.

If we now turn to the last two columns of Table 1, we find the average values of the probable errors to be

$$\overline{R_i} = \pm 0^M.09$$
$$\overline{R_e} = \pm 0^M.12.$$

The goal set forth in this investigation to represent the brightness of comets with the precision of their observations has not quite been reached. In the case of nine comets R_e could be made smaller than R_i. The rest show either irregular variations or semi-periodic changes that could not be taken into account. There is considerable positive correlation between R_e and R_i indicating that the comets with evidence of intrinsic variations are also the same ones that give the greatest departures from the photometric curves.

Periodic Phenomena

When the least squares solution is made for the parameter H_0 and n and the light curve is constructed, it becomes evident that the departures of the observed points from this curve are by no means at random. In some comets, like 1936 *a*, the residuals are negative after perihelion and positive before; in others, like 1915 II, the reverse is true. Still other comets, like 1908 III and 1890 II, have positive residuals at the beginning of the observation, negative in the middle, and again positive at the end. The perihelion passage obviously does not play any part in this variation. These changes in light suggest a definite periodicity, an idea which is not altogether novel. Beyer[18] found a period of 27 days in the variation of brightness of comet 1925 II. Lau[19] suggested a period of 21 days for comet 1914 V. His results were confirmed by Kupffer and Schoenberg.[20] Vorontsov-Veliaminov[21] found a period of 6.5 days with an amplitude of $0^M.4$ for comet 1911 V. It is shown in this investigation that the periodic variation in the light of comets is a rule rather than an exception.

Table 3 contains the results obtained for the periodic variation in the brightness of comets. The column denoted P gives the period in days; column a gives the semi-amplitude in stellar magnitudes; R_e' and R_e are the probable errors of one normal place obtained before and after periodic corrections have been applied; column i gives the quantity $100\ (R_e' - R_e)/R_e'$ which is the percentage of improvement due to the periodic corrections; column t gives the length of time in days during which the comet was photometrically observed; t/P is the fraction of the period of variation covered by the observations, and, finally, $\overline{r}$ is the average heliocentric distance at which the comet was observed.

We notice that the shortest period in Table 3 is 12 days and that the longest is 600 days. Periods shorter than 12 days undoubtedly exist, but their detection would be difficult as the normal places usually cover

a stretch of time of several days. On the other hand, periods longer than 600 days may also exist, but few comets have been observed for a sufficiently long time to make the determination of such periods possible.

The improvement due to the introduction of the periodic variation is sometimes very striking. In comet 1936 *a* the probable error is reduced to one quarter of its original value, and in comets 1862 III and 1925 II to one third.

TABLE 3

INTRINSIC VARIATION IN LIGHT

No.	Comet	P d	a M	R_e' M	R_e M	i	t d	t/P	r
2	1861 II	34	0.27	0.263	0.119	55	57	1.7	1.08
3	1862 III	20	0.19	0.148	0.058	61	38	1.9	1.00
5	1881 III	68	0.27	0.173	0.100	42	118	1.7	1.20
8	1886 IX	36	0.38	0.272	0.175	34	60	1.7	0.83
9	1890 II	336	0.84	0.398	0.175	56	244	0.7	2.28
9	1890 II	88	0.31	0.175	0.088	49	244	2.8	2.28
10	1893 II	52	0.31	0.222	0.117	48	35	0.7	0.79
11	1898 I	100	0.65	0.361	0.140	61	80	0.8	1.32
11	1898 I	24	0.22	0.140	0.086	39	80	3.3	1.32
12	1899 I	36	0.82	0.584	0.390	33	119	3.3	1.21
12	1899 I	64	0.67	0.390	0.185	53	119	1.9	1.21
13	1900 II	12	0.09	0.112	0.102	10	64	5.4	1.11
14	1902 III	18	0.13	0.114	0.081	30	69	3.8	1.20
15	1903 IV	21	0.30	0.147	0.109	26	61	2.9	0.97
16	1904 I	66	0.11	0.115	0.091	21	241	3.8	3.02
18	1907 IV	24	0.33	0.332	0.275	21	227	9.5	1.07
18	1907 IV	72	0.23	0.275	0.256	8	227	3.2	1.07
19	1908 III	76	0.36	0.255	0.121	53	148	2.0	1.46
20	1910 II	480	0.94	0.595	0.357	40	243	0.5	1.34
20	1910 II	80	0.35	0.357	0.223	38	163	2.0	1.80
20	1910 II	96	0.62	0.357	0.223	38	80	0.8	0.96
22	1911 V	104	0.27	0.211	0.136	36	161	1.5	1.04
23	1911 VI	14	0.21	0.170	0.137	19	51	3.6	0.95
24	1912 II	52	0.31	0.207	0.160	23	118	2.3	1.10
25	1913 II	16	0.34	0.253	0.153	40	39	2.4	1.48
26	1913 IV	54	0.57	0.283	0.136	52	58	1.1	1.41
29	1914 V	120	0.23	0.280	0.175	38	442	3.7	1.56
30	1915 II	600	1.08	0.547	0.200	63	317	0.5	1.94
33	1919 III	16	0.27	0.211	0.148	30	41	2.6	0.91
35	1925 I	38	0.27	0.190	0.080	58	73	1.9	1.54
37	1930 III	58	0.28	0.177	0.110	38	68	1.2	0.83
38	1932 V	68	0.27	0.194	0.133	31	59	0.9	1.07
40	1932 X	64	0.51	0.268	0.134	50	64	1.0	1.22
41	1935 I	22	0.17	0.159	0.121	31	68	3.1	0.90
42	1936 a	160	0.33	0.326	0.083	75	98	0.6	1.16
43	1937 b	216	0.36	0.211	0.101	52	205	1.0	1.90
44	1937 c	32	0.36	0.167	0.065	61	59	1.8	0.99
45	1937 f	48	0.12	0.095	0.076	20	65	1.4	0.92

In some cases double periods were clearly indicated. Comets 1890 II, 1898 I, 1907 IV, and 1910 II are in this class. The situation is undoubtedly even more complex than this since in many cases secondary waves in the light curves had a clearly expressed periodic character which, however, did not extend through the whole time of observations. Such cases include comets 1858 VI, 1862 III, 1911 V, 1914 II, 1914 V, and

1915 II. Comet 1910 II had the same fundamental period, 480^d, through the whole range of observations, but the secondary periods before and after perihelion were 80 days and 96 days, respectively. A somewhat different situation was found with comets 1902 III and 1912 II. Their periods were the same during the time covered by the observations, but the amplitude of the variation was gradually diminishing.

From this summary it should be clear that comets do not exhibit the regularity of Cepheids in their variation of light. Their stellar counterparts are rather semi-regular variables, like RV Tauri, in which neither amplitudes nor periods are constant and in which secondary waves make their appearance from time to time in their light curves. We have to deal not with a strictly periodic process but rather with a cyclic change.

As mentioned before, the average probable error of one normal place, $\pm 0^M.12$, is appreciably higher than the average probable error $\pm 0^M.09$ derived from the combination of observations. Some part of this difference is undoubtedly due to short-period fluctuations developing in the light curves which do not persist through the whole interval covered by the observations. Irregular outbursts or suppressions of light must also play some rôle in this difference.

The question may be raised as to the reality of periods such as 600 days for comet 1915 II when the observations cover only half of this time. As an isolated case this comet might be treated as well by splitting the light curve into two parts, before and after perihelion, and evaluating the photometric parameters separately for these parts. The periodic variation, however, is the only scheme that fits all or nearly all comets with a substantial reduction of residuals and of the probable errors of the parameters. From Table 3 we find $R_e' = \pm 0^M.25$ and $R_e = \pm 0^M.13$. This means an improvement of 50 per cent, and thus the introduction of periodic terms reduces the probable errors of normal places to one-half of their former value.

For comet 1937 *b* Beyer[22] finds the same one-sided distribution of the residuals as established in his paper. He ascribes it to the maximum of light being shifted to a time 31 days before perihelion. Such an explanation obviously leaves open the question as to why the maximum of light was shifted. As was mentioned before, the shift occurs in other comets, and in both directions. The answer to the shift in the maximum of light in this particular comet is furnished by the period of variation in light of 216 days.

The last column of Table 3 gives the average heliocentric distance of the comets at which they were observed. There is only a slight positive correlation between P and $\overline{r}$. No pronounced correlation is to be expected as the same comet at the same heliocentric distance may exhibit several co-existent periods.

The correlation between H_0 and P is much more pronounced. The

brighter intrinsically the comet is, the longer the period of its variation in light.

There is a definite correlation between the period and semi-amplitude as should be expected on almost any mechanical model set up to account for the variation in the light of comets. With the increasing period the amplitude is increasing, but the correlation is far from being linear (Figure 3). For longer periods the curve flattens out considerably.

The question now arises as to whether this periodic variation in light is connected with other physical characteristics of comets. The only indication of this connection is the already mentioned observation by Lau where, regarding 1914 V, the secondary waves in the light curve were correlated with the deviation of the tail from radius-vector.

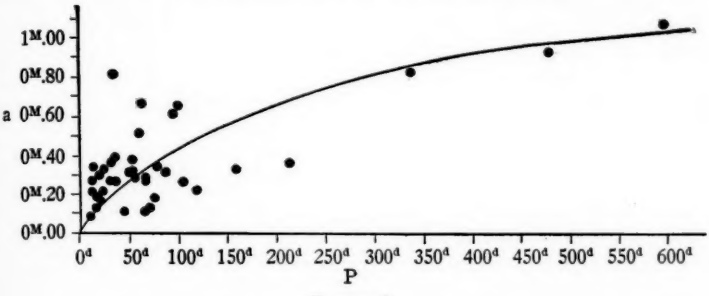

FIGURE 3
Correlation between the period, P, of the variation in brightness and semi-amplitude, a.

The idea of periodic variations in comets was first subjected to mathematical treatment by Bessel in his classic work on Halley's comet of 1835. Bessel's original results were re-investigated by S. V. Orlow[23] who found a period of 4.6 days for the oscillation of an emission sector about its mean position. From the structure of the tail of comet 1908 III Orlow found the periods of oscillation of the emission to be 1.91 days and 3.92 days for September 17 and November 14, respectively. Vorontsov-Veliaminov[24] in a similar investigation of comet 1893 IV found a period of 3.79 days. Bredichin[25] has conclusively shown the reality of the periodic oscillation of emission sectors in comet 1862 III with a period of 2.9 days. It is interesting to note that this oscillation was observed only between August 11 and 31, gradually dying down after the latter date. The periodic oscillation of emission jets allowed Bredichin to explain the unusual γ-shape of the tail of this comet. Even a casual look at the photographs of such comets as 1908 III and 1911 V with their wavy tails strongly suggests a periodic variation in the direction of emission. This idea was first developed by Dunlop[26] who found for comet 1825 I a period of 20 hours. W. H. Pickering found for comet 1892 I[27] a period of four days, and for comet 1906 I,[28] a period of 18 days.

Finally, Horn[29] suggested for comet 1907 IV a period of 16 hours. Since there is no continuous record of observations, such deductions are essentially based on observations separated by the interval of one day and sometimes longer. Consequently, all of these results are hardly to be considered as well established. For the sake of completeness we may mention here that Julius Schmidt found a period of 25.6 days in the variation of the dimensions of the coma of comet 1861 II.

The observational material is obviously insufficient to establish any correlation between the various periodic phenomena in comets.

The existence of periods of different length even in the same comet (as in 1910 II) clearly indicates that the cause of the variation in light must lie within the comet itself rather than in some extraneous object like the sun. All attempts to prove a correlation between the relative numbers and variations in the brightness of comets have been unsuccessful, except, perhaps, for Encke's comet.[30] In this case, however, the conditions of visibility of the comet may well provide a more satisfactory explanation. The very existence of comets like Biela and Brooks (1889 V) which split into several components the brightness of some of which was variable speaks against any solar influence. In Comet Brooks the solid angle at the surface of the sun subtended by the components was but a few minutes of arc. A stream of charged particles or a beam of ultra-violet radiation from the sun affecting one component and missing another is hardly imaginable. The remarkable variation in the brightness and aspect of such a comet as 1925 III (Schwassmann-Wachmann 1, Per.), at a distance of more than six astronomical units, would also be very difficult to explain on the basis of the sun's activity.[31]

It is not easy to suggest any simple scheme to account for the periodic variation in brightness. There can be little question that it is connected in some way with the mechanical processes in the heads of comets.[32] The existence of many components in the nucleus of a comet would require a certain periodic motion about a common center of gravity.[33] With small masses involved, the periods should be rather long and of the order of one hundred days or longer. Shorter periods indicate an oscillatory motion rather than a rotation.

Influence of the Moon

There is another periodic variation in the brightness of comets which has to do with fluctuations owing to the light of the moon. Table 4 gives the semi-amplitudes, a, and the quantities, i, equal to the percentage of the improvement in the probable errors.

It is seen that comparatively few comets show any influence of the moon, a fact which agrees with the results of other investigators. The main reason why only a few comets are affected by the light of the moon is that most comets are not observed near full moon. The most important data for the correlation are therefore seldom available. Another reason

is that a correlation between the phases of the moon and the fluctuations in the brightness of comets depends not only on the phases of the moon but also on the position of the comet in respect to the moon. A comet in the morning sky will be affected differently from a comet in the evening sky. The brightness of the moon, on the other hand, is not simply proportional to the area of its illuminated part, but it depends, too, very appreciably on the phase angle and the geocentric distance of the moon. It is also possible that large diffuse comets are affected by the light of the moon to a greater degree than small compact objects.

TABLE 4

VARIATION DUE TO MOON

Comet	a_M	i
1874 III	0.26	40
1900 II	0.12	14
1911 II	0.48	59
1912 II	0.23	12
1913 II	0.26	12
1914 V	0.08	5
1919 III	0.19	34
1932 VI	0.05	5
1936 a	0.05	5

We notice in Table 4 that semi-amplitudes are rather small and are usually less than $0^M.3$. The only exception is comet 1911 II which perhaps had a period of intrinsic variation approaching that of the synodic month.

The nuclei of comets are probably not affected by the light of the moon to such an extent as their total brightness. Philippov[34] could not find much correlation between the phases of the moon and fluctuations in the brightness of the nuclei of comets 1886 I and 1886 II.

PHASE EFFECT

A few words may be said about the phase effect in comets. Müller[35] found that the brightness of comet 1886 I can be represented by the simple formula $J = J_0/\Delta^2 r^2$, whereas the phase correction in this case would have amounted to $5^M.1$ on Lambert's law. Since that time many attempts have been made to determine the influence of phase upon the total brightness of comets with uniformly negative results. The conclusions reached previously in this study, in so far as the statistical behavior of comets is concerned, are very definite. There is no correlation whatever between the departures from the simple photometric formula and the phase angle. Many cases of individual comets have been discussed for which a large variation of the phase-angle furnished a good test of this conclusion. The results were negative in every case. This is somewht puzzling as a considerable proportion of cometary light is due to scattering. Many comets show evidence of polarization of light not only in their nuclei, heads, and tails, but also in their spectral bands.[36]

The lack of phase effect so far as total brightness of comets is con-

cerned may be considered as proved. This does not mean that the nuclei of comets are not affected by phase. Since, however, the difference between the total brightness of a comet and that of its nucleus is usually several magnitudes, even a large phase effect of the nucleus may pass unnoticed in the study of the total brightness.

General Considerations

It should be clear from this investigation that the attempt to develop a consistent physical theory of comets is hampered by the lack of observational material. Not that the comets have not been observed enough. Astronomical literature is full of observations of the physical characterisics of comets, but this material is mostly of an indefinite and vague kind not amenable to a precise study. Some photometric data on comets are hardly better than the ancient Chinese measures of comet tails in feet and inches. Comets are transient objects, and nothing can be done to improve former observations. In the future well-planned photometric and other observations requiring only modest instrumental means and carried out on a definite plan are highly desirable.[37]

References

[1] *Perkins Observatory Contribution* No. 16, 1942.

[2] *Perkins Observatory Contribution* No. 15, 1941. Abridged exposition of this paper was published in *Pop. Ast.*, **49**, 467, 1941.

[3] R_e is the measure of external consistency, and R_i is the measure of internal consistency. See Birge, "The Calculation of Errors by the Method of Least Squares," *Phys. R.*, **40**, 207, 1932.

[4] "Untersuchungen über die Grösse und Helligkeit der Kometen und ihrer Schweife," *Denschriften der Math.-Naturwiss. Klasse, Kaiserliche Ak. der Wiss.*, Wien, 1896-1917, in five parts.

[5] *Russ. Astr., J.*, **5**, 40, 1928.

[6] *Circ. Poulk. O.*, No. 22-23, 1937.

[7] See, for instance, *A. N.*, **23**, 9, 1854; some doubts as to the applicability of formula (3) were already expressed by Olbers in 1816 (*Gesammelte Werke*, Band **1**, p. 115, 1894).

[8] Olbers, *op. cit.*; W. Herschel, *Scientific Papers*, Vol. 2, p. 403, 1912.

[9] *A. N.*, **131**, 33, 1892.

[10] Holetschek, *A.N.*, **131**, 239, 1892.

[11] *Russ, A. J.*, **6**, 297, 1929.

[12] *Russ. A. J.*, **2**, pt. 3, 68, 1925.

[13] *Poulk. Obs. Circ.*, No. 4, 17, 1932.

[14] *Russ. A. J.*, **2**, pt. 3, 1925. In a later publication (*Circ. O., Poulk*, No. 23-24, 1937), he gives $\overline{n} = 3.90$ for 67 comets, apparently not including into the mean several negative values of n.

[15] "Untersuchungen, etc.," pt. 5, 91, 1917.

[16] Physical data for this comet are discussed in the definitive orbit by Wessel, *A. N.*, **151**, 209, 1900.

[17] *A. N.*, **226**, 271, 1926.

[18] *A. N.*, **227**, 71, 1926.

[19] *B.S.A. Fr.*, **28**, 467, 1914.

[20] *B.S.A. Russ.*, **22**, 187, 1916.

[21] *Russ. A. J.*, **2**, pt. 1, 86, 1925.

[22] *A. N.*, **264**, 401, 1938.

[23] *Russ. A. J.*, **7**, 81, 1930.

[24] *Russ. A. J.*, **7**, 90, 1930.

[25] *Ann. Obs. Moscow*, 2nd ser., **1**, pt. 1, 1886.

[26] *Edinburgh Journ. Sc.*, 1827.
[27] *Harv. Ann.*, **32**, 267, 1900.
[28] *Pop. Ast.*, **15**, 100, 1907.
[29] *Mem. Soc., It.*, **37**, 65, 1908.
[30] See for further references the author's article in *Publ. A. S. Pac.*, **40**, 182, 1928. For later results see Philippov (*Russ. A. J.*, **6**, 70, 1929). He finds small coefficients of correlation and of different sign for different comets. The variation in the brightness of comets due to ultra-violet outbursts on the sun is considered by Richter (*A. N.*, **269**, 103, 1940).
[31] *Pop. Ast.*, **49**, 541, 1941.
[32] Such an idea was proposed by Backlund in connection with Encke's comet (*M. N.*, **70**, 429, 1910).
[33] The periodic motion of meteors about the center of gravity of the comet was considered by Schiaparelli, Charlier, Picart, and Tisserand. For a summary, see Tisserand, *Traité de Méchanique Céleste*, **4**, 258, 1896.
[34] *Russ. A. J.*, **4**, 302, 1927.
[35] *A. N.*, **114**, 361, 1886.
[36] See for further references to this subject: Öhman, *Ann. Obs. Stockholm*, **13**, No. 11, 1941.
[37] See practical suggestions by the present writer for observation of comets, *Pop. Ast.*, **42**, 2, 1934; **49**, 467, 1941; and **50**, 306, 1942.

PERKINS OBSERVATORY, JULY 8, 1942.

Monthly Sunspot Numbers

By WALTER G. BOWERMAN

The following summary which I have noted in regard to the monthly sunspot numbers covering the years 1749 to 1939, a period of 191 years, may be of interest.

In 1810 there were none; so that there remain 190 years in which the incidence by months has been noted. The following tabulation shows the number of times that the respective months had the largest number of spots, for the years on the five pages shown in "Sunspots and Their Effects" (pp. 197-201).

NUMBER OF TIMES THE MONTH HAD MAXIMUM NUMBER OF SUNSPOTS FOR THE YEAR

Month	Page 197	Page 198	Page 199	Page 200	Page 201	Sum
1	3	4	5	4	1	17
2	4	4	5	3	3	19
3	2	3	4	8	3	20
4	1	5	2	4	2	14
5	4	5	2	1	4	16
6	1	2	1	4	2	10
7	1	1	2	5	8	17
8	3	1	2	3	3	12
9	2	1	4	4	2	13
O	2	4	4	0	2	12
N	6	4	3	2	2	17
D	3	5	6	2	7	23
Sum	32	39	40	40	39	190

In the two instances where the number of spots was the same in two months of a given year, the month chosen was alternately earlier and

later. In several years there was a close margin between maximum number and the second largest. The numbers of months as listed in the respective columns above are few; and yet it may not be entirely a chance variation which is recorded. I have no theory to explain the variations, but thought it appropriate to call to your attention the facts as they appear.

Considering first the final column which shows the totals for the entire 190 years; it will be observed that (1) December has the greatest number of maxima, 23; (2) June, six months away, has the fewest, 10; February and March have the next largest numbers, 19 and 20; while six months away are the next fewest; (3) the half year from November to April totals 110 while the half from May to October totals 80. Thus the months which give us our stormy weather in both hemispheres when the sun is nearest to the earth, are the ones which most frequently have sunspot maxima.

When the figures for the respective pages are noted, perhaps the most marked change is the increasing frequency of July balanced by a corresponding decrease in November.

The other characteristic which I noted is the tendency for a given month to appear in consecutive years as the point of maximum sunspots. There are 22 such "twins" and one "triplet." The total number of possible "twins" may be regarded as either 189 or 95; in which case the expected number on the assumption of pure chance would be either 16 or 8, namely, one-twelfth. This also seems like a rather wide divergence to be explained only by chance. Again I offer no supporting theory, but only the facts.

Could it be that the increasing warmth of the earth in the last 80 years is related to the tendency for sunspot maxima to occur in July and August instead of January and February? (19 *vs* 6 and 11 *vs* 18!)

NOTE ON A SUPPOSED ANNUAL PERIOD IN SUNSPOTS

By HARLAN T. STETSON

The question of an annual variation in sunspots raised by Mr. Bowerman's tabulation from my book on "Sunspots and Their Effects" is an old issue sometimes referred to as "An earth effect." The tabulation indicating an apparent maximum in December and a minimum in June would have been further improved had the sunspot numbers of 1940 and 1941 been included since this would have raised the value for August from 12 to 14. A three months' moving average of the monthly values thus revised yields an apparent variation in sunspots with an annual term, the maximum giving an index 50 per cent greater than the minimum value. Mr. Bowerman's method of selecting the data, however, makes the reality of this inference open to question. The main fundamental recurrence in sunspots, with a varying interval of from 9 to 17 years over the period investigated, would seem to preclude the

chance that maxima could be equally distributed throughout all twelve months.

It is interesting to note that Bauer[1, 2] and Clayton[3] from a somewhat smaller amount of data found an annual variation in relative sunspot numbers opposite in phase to that indicated in Mr. Bowerman's tabulations. The maximum to which Mr. Clayton[3] referred occurred near aphelion in July and the minimum near perhelion in January, the evidence in this instance being based on the years 1856 to 1912. Bauer[1] from a study of 172 years found lower values in July than in January, but stressed the tendency for maxima to occur at the equinoxes as is characteristic of the earth's annual magnetic variation. In another paper[2] dealing with the activity of the sun and the earth's magnetism, Bauer again calls attention to the equinoctial maxima in both sunspot numbers and magnetic activity. In addition to the double periodicity for the cycle 1913-1922 he states, "there is some indication, for the cycle considered, of a single annual periodicity in the sunspot number with a maximum near June and a minimum near January," and comments that the mean sunspot number for this cycle was somewhat less through the period when the earth was nearest the sun than during the period when it was farthest away. This is somewhat at variance with results from the 172 year series but is more in accordance with Clayton's findings.

Chapman and Bartels[4] have stated that the conclusions found by Bauer and Clayton are spurious making the following statement.

"For example, L. A. Bauer and, following him, H. H. Clayton, concluded that there is a pronounced annual variation in the relative sunspot numbers, the maximum being near aphelion, in July, and the minimum near perihelion, in January; they called this an 'earth effect' on sunspots. The evidence for this was that, in the mean of the years 1913-1922, the mean sunspot number for the northern summer half-year (April-September) was 46 as against 42 for the winter half-year. But this was only an effect of curvature in the variation over the whole period 1913-22; this period coincided with the rise to, and fall from, the sunspot-maximum of 1917; the apparent mean yearly variation derived from such a set of years must be convex, and will give a higher mean value for the months April to September, which are in the middle of the selected yearly intervals, than for the other six months."

This statement, which appears to confuse Bauer's two papers and that of Mr. Clayton, appears somewhat misleading in view of the statements by Bauer and Clayton themselves.

Apropos of the above it seemed worthwhile to take the actual means of the relative sunspot numbers for each month of the years contained in the table from 1749 extending through December, 1941, to which Mr. Bowerman refers. Since it is generally known that the earlier values from 1749 to about 1820 are less reliable, the means have been tabulated

both for the 193 year interval and for the 121 year period excluding the data prior to 1821. The results are printed herewith.

MONTHLY MEANS OF SUNSPOT NUMBERS

Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
(1749-1941)											
43.7	46.3	44.9	45.1	46.7	46.1	46.1	46.0	45.8	46.0	45.8	45.9
(1821-1941)											
43.4	46.9	46.5	45.4	45.7	46.5	46.5	46.9	45.7	46.4	44.3	45.4

It will be noted that here a minimum value occurs in January with no indication of a reduction in June as in Mr. Bowerman's conclusions. The extreme variation in the second series from January to August is a less than 8 per cent-change in sunspot numbers.

Mr. Clayton has attributed low winter (northern hemisphere) values in his discussion to cloudiness and prevalent poor seeing during the winter months which may be a factor in his apparent annual variation.

The tabulation does not appear to substantiate very strongly the equinoctial maxima. There is, however, a well-defined semi-annual variation in the occurrence of polar lights to which C. F. Brooks and I have called attention.[5] The semi-annual variation in the characteristics of the earth's magnetism also appears beyond doubt. Polar lights and terrestrial magnetism are associated with sunspots, but the grounds for the semi-annual variation would seem to be of a positional nature rather than attributable to any equinoctial variation in the sunspots themselves. For the relatively long interval considered there still appears to be but little evidence for any persistent annual period in sunspots that can be justifiably associated with the earth's perihelion or aphelion.

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[3] H. H. Clayton, "World Weather Records," *Smithsonian Miscellaneous Collection*, **79**, v, 1927.

[4] S. Chapman and J. Bartels, *Geomagnetism*, Vol. II, 548, 1940.

[5] H. T. Stetson and C. F. Brooks, "Auroras Observed at the Blue Hill Observatory," *Terrestrial Magnetism*, **47**, 21-29, 1942.

Planet Notes for December, 1942

By R. S. ZUG

NOTE: Greenwich Civil Time is employed unless otherwise stated. To obtain Eastern Standard Time subtract 5 hours, Central Standard Time, 6 hours, etc. The planetary phenomena are described as they are to be seen from latitude 45° N. The data are taken chiefly from *The American Ephemeris and Nautical Almanac.*

Sun. Apparent positions of the sun for December 1, 1942, and January 1, 1943, respectively, are: $\alpha = 16^h\ 25^m.6$, $\delta = -21°\ 40'.3$; $\alpha = 18^h\ 42^m.2$, $\delta = -23°\ 5'.6$. December 1 finds the sun moving through the constellation Ophiuchus. On

December 18 it enters the constellation Sagittarius. The sun reaches the winter solstice December 22, $11^h\ 40^m$, which instant marks the beginning of the season of winter.

Moon. Phenomena of the moon will occur as follows:

		h	m
Last Quarter	Dec.	1	1 37
New Moon		8	1 59
First Quarter		14	17 47
Full Moon		22	15 3
Last Quarter		30	18 37
Perigee	Dec.	9	0
Apogee		23	23

Mercury. Mercury attains superior conjunction on December 1, and will accordingly be an evening object during December. The planet will remain inconspicuous due to its southerly declination.

Venus. The planet Venus is now an evening star, having passed superior conjunction on November 16. By the end of December it may be possible to locate the planet just after sunset, but this will be difficult due to the planet's southerly declination of —23°.

Mars. Mars is to be found in the morning sky. By the end of December the planet will be 2^h west of the sun and in declination —22°.

Jupiter. Jupiter is a morning object in the constellation Gemini. The apparent motion of the planet is now retrograde.

Saturn. Saturn reaches opposition on December 2. The planet is situated in the constellation Taurus, within which it is in retrograde motion just north of the Hyades.

Uranus. Apparent positions of Uranus for December 1, 1942, and January 1, 1943, respectively, are: $\alpha = 4^h\ 1^m2$, $\delta = +20°\ 28'7$; $\alpha = 3^h\ 56^m3$, $\delta = +20°\ 15'1$. Opposition occurred November 25.

Neptune. Neptune reaches western quadrature on December 24. The planet is now situated about midway between the stars 10 Virginis and 13 Virginis.

Asteroid Notes

By HUGH S. RICE

CORRECTION—In the October "Asteroid Notes" it was stated (page 435, paragraph 2) that Vesta was located with binoculars from indoors in about "5 minutes". This should read: "5 seconds".

Since that time we have done better than that. From our station in New York City, we have picked up Vesta from indoors with the 18x binoculars held in the hand with no fixed support, in less than 5 seconds, without opening the window at all. These instances are mentioned because of the common report that the minor planets take a long time to locate.

During summer vacation time, Vesta was located and observed by several members of the Junior Astronomy Club of this city, using the club's 2⅞-inch telescope.

Vesta is still observable in the evening sky for the remainder of the year, and can be observed with field-glass or small telescope. It is still bright for an asteroid. It is moving rather fast in its apparent path and direct motion. We

have seen a displacement in its position in the sky, in less than a hour's time between two observations, using only 18 diameters of magnification.

The position of Vesta can be plotted on a celestial atlas with the aid of the data of the conjunctions, etc., given here, the points falling nearly on a straight line. The minor planet's magnitude ranges from 7.5 on November 9 to 7.8 on December 3.

On October 29.7 (U.T.) Vesta is 2⅝° south of the star 17 Capricorni. On November 5.0 it is 2¾° southeast of 17. On November 15.5 it is 1¼° south of χ Capricorni. On November 20.3 it is 1¼° south of ϕ. On November 25.0, 1½° northwest of ζ. On November 27.8, less than ¼° north of 35 Capricorni. On December 4.5, Vesta is 0°.7 south of ϵ. At the end of the month it runs off the Córdoba charts and goes back onto the Beyer-Graff series. Just before 0^h December 3, Vesta is 0°.3 south of 37 Capricorni, between two other stars (of magnitude 7), one north and the other south of the asteroid. On December 7, at 0^h Vesta's position practically coincides with that of a 6th-magnitude star nearly 1° south and to the west of κ Capricorni.

Hayden Planetarium, American Museum of Natural History,
New York, New York, October 21, 1942.

Occultation Predictions for December, 1942

(Taken from the *American Ephemeris*)

Date 1942	Star	Mag.	Immersion: Greenwich C.T.	Immersion: a	Immersion: b	Immersion: Angle E from N	Emersion: Greenwich C.T.	Emersion: a	Emersion: b	Emersion: Angle E from N
			h m	m	m	°	h m	m	m	°
Occultations Visible in Longitude +72° 30′, Latitude +42° 30′										
Dec. 3	γ Vir. *m*	2.9	10 20.2	−1.4	+0.2	114	11 37.8	−1.5	−0.5	301
3	BD−0° 2603	6.1	11 36.6	−2.1	+0.6	89	12 46.8	−1.2	−1.9	328
15	14 Cet	5.9	20 17.2	−0.9	+2.0	58	21 31.1	−1.4	+1.4	252
20	179 B.Tau	6.0	8 5.4	+0.2	−4.2	144	8 36.3	−0.9	+1.8	202
21	318 B.Tau	5.7	8 10.2	..	..	155	8 36.3	..	..	200
26	o^1 Cnc	5.2	0 59.8	0.0	+0.4	122	1 53.9	0.0	+1.9	249
26	o^2 Cnc	5.6	1 10.0	+0.4	+2.4	56	1 56.9	−0.5	−0.1	315
Occultations Visible in Longitude +91° 0′, Latitude +40° 0′										
Dec. 3	γ Vir. *m*	2.9	10 2.9	−0.6	−0.5	138	11 8.3	−1.4	+1.0	272
3	BD−0° 2603	6.1	11 6.2	−1.2	+0.1	118	12 22.9	−1.5	−0.2	296
14	*h* Aqr. *m*	5.6	3 44.8	−0.3	+0.7	34	4 36.8	−0.5	−1.8	280
15	27 Psc.	5.1	4 46.7	−1.0	−3.1	123	5 23.2	0.0	+2.1	192
18	389 B.Cet	6.3	7 40.3	−0.6	+1.0	31	8 25.5	0.0	−2.7	299
19	BD+11° 445	5.9	2 9.8	..	..	132	2 46.0	..	..	180
Occultations Visible in Longitude +120° 0′, Latitude +36° 0′										
Dec. 3	γ Vir. *m*	2.9	10 0.9	0.0	−2.6	170	10 30.2	−0.4	+3.6	230
3	BD−0° 2603	6.1	10 49.8	−0.2	−1.2	152	11 38.8	−0.9	+2.0	252
12	29 Cap.	5.5	4 10.6	−0.2	+0.4	41	5 1.8	−0.5	−1.5	280
14	*h* Aqr. *m*	5.6	3 27.8	..	..	4	4 7.5	..	..	301
15	27 Psc	5.1	4 3.8	−2.2	−1.1	98	5 7.4	−0.7	+1.6	206
15	29 Psc	5.2	6 12.6	−0.9	−0.5	72	7 18.1	−0.4	−0.2	241
17	ν Psc	4.7	7 5.7	−1.5	−1.7	103	8 6.9	−0.8	+1.1	214
18	389 B.Cet	6.3	7 6.6	−1.4	+1.2	43	8 17.2	−1.3	−1.7	277
19	BD+11° 445	5.9	1 8.9	−1.0	+1.3	93	2 13.7	−0.7	+2.5	220
25	d^2 Cnc	6.2	11 21.4	−2.9	+1.2	61	12 22.6	−0.7	−3.9	334
26	81 Cnc	6.4	9 6.4	−2.4	+1.8	74	10 24.9	−2.0	−2.4	319
31	65 Vir	5.9	15 50.4	−2.1	−0.8	100	17 9.3	−1.4	−2.1	313

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

Since the writing of the last Meteor Notes the Perseids have come and gone. Much of the country east of the Mississippi, where so many of our observers live, had abominably cloudy weather for the days near maximum, and the results were often discouragingly few. For instance, many groups which had planned simultaneous observations from two stations found no chance to put their plans into effect. Even when, at a given place, the clouds broke for a few hours, conditions were rarely excellent. Fortunately members farther away had better weather and from their reports I would infer that this last return gave a very good, perhaps excellent display, as Perseid showers go. When the returns are more completely in, as usual I shall publish the details of the observations, with further comments on the results.

For myself, I had the great pleasure of being invited with my wife to be guests in the Catskills at "Lavorika" the hospitable home of Mr. John A. Kingsbury, a very good friend of mine and a fellow A.M.S. member. We were there for the nights of August 11/12 and 12/13. Unfortunately the second was wholly cloudy, the first only partly clear until about 13 hours. Mr. and Mrs. Kingsbury on this first night gave one of their famous "meteor parties." Probably 200 people were present, about a fourth on personal and the rest on general invitation, but all interested enough to come up the mountain (the height of "Lavorika" is 1800 feet) and learn something about the Perseids and see what they could of them. Had the weather during the day been clear, probably many more would have been present. As Woodstock, New York, which is a famous artist center, is near, many most interesting people were among the group. Also I had the pleasure of seeing some who assisted us there in 1931 when we saw the splendid Leonid display of that year. (See POPULAR ASTRONOMY **39**, 605, 1931 and **40**, 46, 1932.) The party started at sunset, the part dealing with meteors—talks by Mr. Kingsbury and myself—about dusk, and the observing when it became really dark. Clouds stopped work about 1:00 A.M., but the groups did not wholly disperse until about 6:00 A.M. I observed 40 meteors in all, most rather bright, for the sky was never wholly clear so that few faint ones could be seen. The best single count was by Mr. C. J. McCarthy, who was one of our observing group in 1931, and he saw 70. Incidentally my own observing was frequently interrupted to reply to questions and keep things going, so my total was no fair sample of

what the hourly rate really was. The actual statistics of our group will be included in the tables covering the 1942 Perseids.

This unique idea of Mr. Kingsbury to combine serious observing with a most agreeable social gathering is one I can heartily commend to those of our members with a well-situated place in the country. I do not believe any who were present ever forget these meteor parties. In fact I have had the pleasure of being at three and each has made me the more anxious to go again.

As to the chief fall showers, the Moon is full on October 23, four days after the Orionid maximum, so meteors should be well observable for the last 3 hours of the night of October 19/20. The Moon is at first quarter on November 15, so that any Leonids which come this year will not be much troubled by moonlight. Conditions are even slightly better for the Geminids, with maximum on December 12/13, as first quarter comes two days later. It is therefore hoped that all of our members will make real efforts to observe these three showers, only one of which comes in a given month, as moonlight conditions in 1942 are very favorable for two out of the three.

Fireball of 1934 April 5/6

From the *Hydrographic Bulletins* of 1934, two observations of this fireball are available, but only salient facts will be copied here. S1 is the Am. S.S. F. C. Voslah, in λ 86° 52′ W, ϕ 25° 52′ N, observer First Officer T. R. Merritts: ". . . brilliant meteor . . . at 1025 GMT . . . became visible (near) Enif bearing 90° . . . altitude about 28°, travelled in an irregular course to the southeastward, fading below Venus bearing 100°, true, . . . altitude about 10° . . . was brilliant white, three times the size of Venus . . . visible about 2½ seconds . . . left a pale reddish trail . .". S2 is the Am. S.S. Yorba Linda in λ 87° 25′ W, ϕ 27° 48′ N, 1023 GMT, First Officer C. O. Carlson: ". . . a meteor . . . altitude 30°, bearing about 125° true. The body when first sighted appeared small and left a white trail. After travelling downward for about a second it suddenly burst in a bright white flare leaving fragments of white light on both sides of its track. The meteor then turned red and disappeared at an altitude of 10° . .".

There being no doubt that these reports concerned the same meteor, a graphical solution was made. It was only possible to compute the end height by assuming that the meteor had a perpendicular path from S2, as the word "downward" is used and no second bearing is given. On this insecure assumption, heights are derived. The observations from S1 could be partly checked on a celestial globe and appear the more carefully made. They have hence been given double weight.

Beginning height from S1 140 km, from S2 228 km, wt. mean 169 km
End height from S1 71 km, from S2 104 km, wt. mean 82 km.

The results are too uncertain to justify deriving other data, and must be considered of low weight.

Fireball of 1934 January 11/12

On the evening of this date a fireball was seen from Flower Observatory by my wife and myself. As it was comparable to Jupiter in brightness and ended low in the S. E., it seemed probable that it was a brighter object as seen from New Jersey, and hence to be worth the publicity necessary to obtain reports from others. The Philadelphia papers and the Associated Press cooperated so well that the request was generally published over a wide territory and 118 reports came in. Unfortunately, there was none from ships though the path lay wholly over the Atlantic, east of New Jersey.

By a strange chance, four A.M.S. members saw it: C. P. Olivier at Flower Observatory, S201; F. W. Smith at Glenolden, Pennsylvania., S207; G. P. Kirkpatrick at Piermont, New York, S264; and A. Schenckman at New Market, New Jersey, S107. Also Mrs. J. B. Milano, Wilmington, Delaware, S252, gave a plot with respect to bright stars, and several others used stars as reference points. We were further aided by numerous observers near Newark, New Jersey, as well as some east and west of that city, who gave the path as vertical or nearly so. This was an excellent check on the azimuth of the path which was derived in the usual graphical way. A brief preliminary note, prepared with the assistance of Mrs. F. V. Wills then on our staff, and based upon the observations at S201 and S264, was published in the *Philadelphia Inquirer* within a few days, giving approximate heights, etc. The data were then laid aside and only again taken up recently. To my intense regret my own observation alone is missing. Only the magnitude from the newpaper and the coordinates from the computation sheet of Mrs. Wills are available. A search in all likely places has failed to produce the original, and as there are literally tens of thousands of letters, maps, reports, and record sheets on meteors here, once a single sheet gets out of place, it is almost impossible to locate it. Usually, this loss would not matter when the coordinates are available but the solution shows that, while my end point fits the prolonged path derived from the mean of the other observations in both azimuth and linear height fairly well, yet it is far lower than the rest, thus giving a longer path. The explanation probably is (though my memory fails me on the point) that the others observed the explosion point, or rather where the object partially broke up as the end, while I saw fragments go farther. Fortunately the radiant is in no way affected, only the path length and height of the end point. I have had to assume that all other observations of the "end point" really refer to the "explosion" point, and that my own refer to where the last fragments disappeared. A partial check on slope of the path and heights of certain points thereon was furnished by observers at 3 stations who did not see the last part of the path due to obstructions.

Curiously, though there were so many reports, few attempted to give the color. On the whole, blue-white estimates predominated. From Philadelphia, the object was about as bright as Jupiter, to observers in eastern New Jersey, certainly brighter than Venus. It was in sight about 3.2 seconds (duration uncertain), was pear-shaped, and several observers state that they saw a smaller body just behind the larger. Some observers mention the path as slightly curved or as an arc.

With the exception mentioned above due to my own observation, the solution is satisfactory and more confidence than usual can be placed in the results. The usual data follow. Parabolic heliocentric velocity has been assumed in deriving the zenith correction.

Date	1934 Jan. 11/12, $8^h\ 02^m\ 25^s$ P.M., E.S.T.		
Sidereal time at explosion point	53°		
Began over	73° 34′ W, 40° 03′ N	at	104 ± 20 km[1]
Exploded over	73° 13′ W, 39° 31′ N	at	48 ± 14 km[2]
Last fragments ended over	73° 04′ W, 39° 18′ N	at	18 ± 5 km[3]
Length of path to explosion point			48 km
Length of whole path			88 km
Projected length to explosion point			37 km

[1] 11 observers. [2] 8 observers. [3] 1 observer.

Projected length of whole path	68 km
Observed velocity	Low
Radiant (uncorrected)	$a = 153°$, $h = 40°$; $\alpha = 333°$, $\delta = +69°$
Zenith correction	$-3° 44'$
Radiant (corrected)	$a = 153°$, $h = 36° 16'$; $\alpha = 323°$, $\delta = +68°5$

No fireball anywhere near the date in question is found in the von Niessl-Hoffmeister Catalogue. It is doubtful if the fireball was large enough to furnish meteorites despite the low height of the last fragments. In any case they would have fallen into the ocean.

Flower Observatory of the University of Pennsylvania, Upper Darby, Pennsylvania, 1942 September 17.

Fireball of 1936 October 21

By Donald A. MacRae

The fireball appeared in the vicinity of the nation's capital well after dark on this October evening. It began over the southern border of Pennsylvania, a hundred miles west of Philadelphia, and traveled in a southeasterly direction, ending over Chesapeake Bay near the mouth of the Potomac River.

Well over half a hundred individual reports of this fireball reached the headquarters of the Society, a considerably larger number than usual. Many were from points near the path, especially from Baltimore and its environs. People in Philadelphia noticed it because it appeared in the southwestern sky. Although the altitude was very low, we have one important observation from near New York City.

Mr. Paul S. Watson, S1, a member of the American Meteor Society, along with Mr. J. L. Woods and Mr. J. B. Field, was at Mr. Wood's Observatory at Sykesville, Maryland, at the time. He writes that he got a good determination of the end point, but that since the path was almost directly overhead he did not see the beginning point. His value for the point where he first saw it gives a valuable check on the direction. His observations were given with respect to the stars, in equatorial coordinates. Another report of high weight was that of Mr. J. Maron Joseph, S2, from Chester, Pennsylvania, which is not far from Philadelphia. He was conducting an observation program with seventeen boys from Smedley Junior High School at the time of the occurrence of the fireball. A home-made altazimuth device was used to determine the coordinates of the beginning and end points. The observer from New York was Mr. E. H. Christman, S19. For him the fireball appeared near the moon so that from his plot on one of the regular meteor charts accurate altitudes and azimuths can be obtained. Of the host of other observations only two will be mentioned in particular, although almost every report was used in one way or another in the final solution. These two were remarkably similar, being made from two street intersections in Philadelphia within a few blocks of one another. One was by Miss Catherine Michel, S10, and the other was by Mr. George H. Gibson of the Philadelphia Bureau of Police, S9. Each of them described exactly where they had been standing and the position with respect to nearby buildings where the fireball was last seen. It was thus possible to go to the spot and obtain an accurate height for a point near the end of the path. As will be seen in what follows this was important in making the solution.

From Mr. Watson's observation, and others, the azimuth of the fireball's path

and the position of the beginning point could be fairly well determined. With these fixed, the end point could be obtained from Mr. Joseph's or Mr. Christman's azimuth of end point. These unfortunately gave values which differed by 30% in the length of the path. For a first solution the altazimuth observations from Chester were adopted, the New York ones being rejected because of the extremely low altitude, and heights for beginning and end points were computed. It was found, however, that the trustworthy heights from Philadelphia, referred to above, gave values only half as great as the mean of the other observers' in this solution. Consequently a second solution was carried out giving full weight to Mr. Christman's observation and assuming that the High School group's azimuths were off by some twenty degrees, presumably due to an error in the north-south line. This brought many of the other observations into line so that it was chosen as the basis for the final solution. It should be pointed out that an error such as that assumed above is very easy to make, especially in an urban area where the streets run almost, but not exactly, north and south. Mr. Joseph's observations, especially the altitudes, were still exceedingly valuable.

The Sykesville observers estimated the brightness as minus six magnitudes, and the apparent angular diameter as about fifteen minutes of arc, a value in good agreement with many other estimates. No one reported seeing a train. Almost everyone remarked on the brilliant white or bluish-white color and many added that the following portions were reddish or copper colored. The fireball burst at the end of its path. Its duration was probably about four seconds, from which the velocity given amongst the detailed results below was computed. Parabolic heliocentric velocity was assumed for the zenith correction.

Date	1936 October 21 8:25 P.M., E.S.T.
Sidereal time of end point	338°
Began over	λ 77° 05′ W, ϕ 39° 43′ N at 145 km ± 20 (a.d.)
Ended over	λ 76° 15′ W, ϕ 38° 17′ N at 51 km ± 6 (a.d.)
Length of path	199 km.
Projected length of path	175 km.
Duration	4 secs. ± 1.5.
Apparent velocity	50 km. per sec.
Radiant (uncorrected)	$a = 156°$, $h = 28°.3$; $\alpha = 228°$, $\delta = 68°$
Radiant (corrected)	$a = 156°$, $h = 22°.3$; $\alpha = 217°$, $\delta = 64°$

Cook Observatory, University of Pennsylvania, June, 1942.

Contributions of the Society for Research on Meteorites

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Concerning the Meteor Observed on January 30, 1868, which Fell through the Atmosphere as a Shower of Stones at Pultusk In the Kingdom of Poland

By J. G. GALLE [1812-1910]

Translated by WALTER H. HAAS

[*Continued from the October Issue*]

KOCK IN SOUTHEAST POLAND

An observation by Dr. L. Neugebauer (a practising physician of Warsaw), made in the vicinity of Lublin, in the Kingdom of Poland, is located in essentially another direction in the sky and hence gives valuable additional evidence. This observation had already been published in part in public pamphlets; Dr. Neugebauer had the courtesy to report to me about it in detail. He writes: "I observed the aerolite on the occasion of a journey I made in the vicinity of Lublin. At the moment it appeared, I was on a level plain with a completely unobstructed view and had the town of Kock [Kotzk] at a distance of 2 versts from me and almost due north. I was 110 versts from Warsaw, which lay in the same direction. The sky was slightly clouded toward the south and southeast but was elsewhere very clear. The Moon and Venus (the latter with a star [planet], Jupiter, likewise very bright, close above it) were on my left; the former was somewhat behind me, so that I shall say it was in the southwest and at a height of 50° above the horizon; the latter was some degrees farther north and about 5° above the horizon. Since I was looking actually at Venus and its companion when the meteor flared up in the firmament, I could thus observe it very exactly from beginning to end. It first appeared to my gaze at a height of about 50° (or probably, instead, somewhat less) above the western horizon, at a horizontal distance of about 30° north of the Moon, as an intensely white, luminous ball, the apparent size of a large cannon ball. The ball moved immediately (leaving behind it a long, broad, magnificently white, luminous train) in a slightly curved arc about 30° long toward the north, rapidly dropping toward the Earth. It disappeared suddenly from my sight, having reached a height of about 10° above the horizon. When it so vanished, its image dissolved ever so suddenly into a magnificent, ruby-red light, radiating far outward, which the next instant changed to a beautiful bright green color and then disappeared. The whole phenomenon lasted about 3 seconds. I did not notice a detonation, but this fact was due perhaps to the noise caused by the sleigh on which I was riding, and to the horses." Dr. Neugebauer somewhat later submitted, in addition, a drawing he made of the phenomenon, from which I infer for the present purpose that the inclination to the horizon of the bright train left behind by the meteor amounted to 30°.

In order to compare next the angle of inclination of the train, which marked the path traversed by the meteor, with the path earlier calculated, the apparent heights of the beginning and end points of the meteor, as these must have appeared when seen from Kock (at horizontal distances of 28 and 19 miles and at elevations above the Earth's surface of 24 and 5.6 miles), are to be computed. One finds 40° and 16°, respectively, as the apparent heights. The difference in azimuth of the 2 points is 46°. From these data there follows an angle of inclination of the apparent path of 33°, differing accordingly by only 3° from the drawing made on the basis of the eye estimate. Therefore, this result may be considered as at least approximately a confirmation of the inclination of the real path as computed on the basis chiefly of the Danzig observation. Furthermore, this confirmation rests on an observation made at a station oppositely located with regard to the meteor.

In making comparison with the remaining estimates relating to the course of the meteor, it is to be observed, by way of introduction, that the Moon was at azimuth 50° and height 33°, Venus and Jupiter close together at azimuth 72°

and height 4°; further, that the beginning and end points of the path, by the preceding calculation, had the azimuths 104° and 150°, respectively, and the heights 40° and 16°, respectively. One now finds the distance of the Moon from the beginning point of the path as 43° and the distance from the beginning point to the end point, or the apparent length of the meteor's path, as 46°. Dr. Neugebauer therefore estimated heights somewhat too large (as is usual in the lower half of the sky), namely that of the Moon at 50° and likewise that of the meteor at 50° (instead of 33° and 40°). On the other hand, he makes horizontal distances too small, namely, that from the Moon to the beginning point, 30°, and likewise that from the beginning point to the end point, 30° (instead of 43° and 46°). If one neglects the *numerical* value of the estimates and the resulting unavoidable errors, the heights of the Moon and the beginning point of the meteor do, in fact, differ little from each other, and, likewise, the beginning point lies nearly equally distant in azimuth from the end point and from the Moon. That changes of the calculated path, of any considerable size, are not admissible is therefore confirmed also by these estimates.

Warsaw

If one should suppose the exactly vertical motion of the meteor as it appeared at Breslau to be uncertain and, in conformity with the initial reports from Pleschen, should place the beginning farther north and hence shift the path more nearly to an east-west line, then this act would be actually incompatible with the observation at Kock, where the beginning was seen above the western horizon. Still less would it agree with the reports from Warsaw, at which locality, indeed, no exact observation is known, but where the direction of the beginning point appears to have been very nearly west-southwest. Mr. Kowalczyk, an observer (at the Warsaw Observatory), writes the following from Warsaw: "The statements of the newspapers in this place, regarding the course of the meteor through Virgo, Hercules, Draco, Ursa Major, and Andromeda (which statements, burdened by contradictions, were also transferred into several German newspapers), are to be characterized as completely erroneous and as depending upon a thoroly unreliable source. According to more reliable reports, I have been able to establish that here at Warsaw the meteor appeared in the neighborhood of the Moon, Jupiter, and Venus, and, indeed, nearer to the 2 planets, so that the beginning was seen at a height of approximately 15° and at an azimuth of 60°-65°. After the meteor had reached its greatest height, which was estimated at not much above 45°, it passed north of the zenith and again rapidly approached the horizon. Statements about the end of the phenomenon agree that the bursting occurred very close to the horizon and, in fact, at a height of about 5° and an azimuth of 17° east of north." A letter from Mr. Deike (likewise of the Warsaw Observatory), in the *Austrian Journal for Meteorology*, III, p. 138, reports similarly: "On January 30, at 7^h, Warsaw Civil Time, there appeared in the southwestern sky, just where the Moon stood, a blazing meteor, like a first-magnitude star of a reddish color, which took the direction from southwest to northeast. After it had traveled some degrees, it began to leave behind a train like the train of an ordinary shooting star. Then, however, the nucleus, as well as the train, began to increase, so that, on the meridian, the diameter of the nucleus had already reached about 20 minutes of arc [20′], and the train had become about 10° long and 2°-3° broad. The light emanating from the meteor in this position was so uncommonly intense that, in comparison with it, the gas light in the street disappeared completely and even

the brighter stars in the sky became invisible. The color of the light surrounding the nucleus, like that of the train, was pale blue or, as others contend, greenish. Moreover, the main nucleus was not directly connected with the train, but was separated from it by a narrow zone which shone with the same light (same color) as the train. Light of a reddish color constantly radiated from the center of the nucleus itself. The meteor dropped to within about 10° of the horizon and, at an azimuth of about 20°, it dispersed into lively sparks, which were extinguished before they reached the Earth. The duration of the flight was 5 seconds."

According to the path calculated earlier, the meteor must, in fact, first have *flared up* at Warsaw at an azimuth between 70° and 80°, *i.e.*, almost in the direction of Venus and Jupiter, while the Moon stood considerably farther to the left. Or, if, according to H. Deike's report, the meteor was the same as a shooting star at the very beginning, it must have originated between Venus and the Moon. However, in no case can its nearness to Venus and Jupiter imply the same altitude, since an initial apparent height of 15° completely contradicts the remaining observations. Rather, the meteor must suddenly have appeared at a height of more than 50°; still rising somewhat, it then passed over the whole western sky to a position in the north-northwest; it then moved downward more rapidly toward the north-northeast. As regards the height of the bursting point, the preceding statements of only 5° or 10°—if based upon otherwise reliable estimates—cannot relate to the chief explosion, but must represent either merely the last explosion of the main mass (which was no longer visible at greater distances) or, more probably, the final flare-up of the only slightly luminous falling masses. I reached complete certainty about this matter through an observation, which will be discussed later on, of the Director of the Evangelical Academy at Warsaw, Mr. v. Bäckmann. I am indebted to Dr. Neugebauer for first calling my attention to this observation. Later Mr. v. Bäckmann, at my request, had the goodness to transmit through Dr. Neugebauer, a supplementary statement, which related particularly to the height of the bursting point. On the basis of an exact measurement carried out by Mr. v. Bäckmann, the houses above which the meteor burst had a height of 10° 40′ as seen from the place of observation. This value would be accordingly the minimum possible altitude; but the altitude of the bursting point must have much surpassed 10° 40′, since Mr. v. Bäckmann remarks: "But if one adds, moreover, that I perceived the bursting of the meteor at a very considerable height above the houses; that, in particular, I saw the sparks of light, which became visible when the meteor burst, fall in long arcs (similar to those of a bursting rocket) and finally vanish still high in the air, then there is no doubt whatever that the bursting point was far more than 10° above the horizon." If one employs basically the height of 5.6 miles above the Earth's surface, found previously, then one finds that the angular height of the bursting point was about 35°. If this angular height is assumed to be 5°-10°, then a real height of about one geographical mile is obtained, which agrees with the height at which extinction was observed in the region of the fall, as will be mentioned later on, but which is to be distinguished certainly from the [height of the] chief explosion and dispersion which marked the termination of the luminosity of the phenomenon as seen at places ***remote*** from the area of fall.

Sokolow, at the Edge of the Territory of the Fall

That the meteor must have appeared initially at a great apparent height in the neighborhood of Warsaw is testified to likewise by an observation made at

the southeast edge of the territory, where the stones fell. According to the report of Mr. Deike of the Warsaw Observatory (as Mr. Kowalczyk reported to me and as Professor Karlinski also contends, in the *Austrian Journal for Meteorology*, III, p. 139), an inhabitant of the village of Sokolow, at longitude [east] 39° 5′ from Ferro and latitude [north] 52° 45′, declared the following about the meteor: "A small star began to recede from the Moon and simultaneously enlarged itself, rising ever higher and higher; it left behind a fiery and likewise enlarging train. The star soon afterwards took a direction slanting toward the horizon and burst, scattering small sparks in all directions; only a white zigzag-shaped cloudlet suspended in the air was left of the phenomenon." The height of the place where the meteor burst was, incidentally, 45°, azimuth about 15°, west of north. According to the earlier path-calculation, the first bright flaring-up of the meteor must have commenced at a height of 54° (its first appearance as a shooting star having been somewhat lower still), *i.e.*, 20° (or somewhat less) above the Moon and some degrees to the right of it. The further motion must have been an ascent toward the zenith and hence a recession from the Moon, agreeing with the statement of the observer. Finally, with the checking of the flight and the dispersion of the meteorite, a change of direction and a steeper descent apparently occurred. The end point must have appeared actually in the north-northwest from Sokolow. For the rest, the statements about the height of the so-called bursting point differ considerably from those relating to places of observation located near this point (where the light of the falling pieces was perceptible down to a lower elevation), as has already been mentioned in connection with the Warsaw observation and as will be brought out in greater detail in the sequel. Finally, in view of this observation at Sokolow, there can be no doubt that the direction in the sky from which the meteor came lay between southwest and west-southwest, a result in harmony with the apparently vertical motion observed at Breslau.

Krakau and Galicia

Some observations from the region of Krakau and Galicia have become known to me through a courteous communication from Professor Karlinski, Director of the Observatory at Krakau. However, these only confirm, in general, the directions in the sky already known and supply nothing new or more exact for purposes of computation. The observations at Krakau place the end point somewhat too far toward the northeast (instead of only north-northeast). At Rzesow, in Galicia, the direction was north. At Lemberg, the direction was northwest (according to the report of Professor Handl in the *Austrian Journal for Meteorology*, III, p. 124). The light was bluish and was found similar to the light of magnesium, by some observers. However, the sky was covered by clouds. No detonation was heard. "At Makow (vicinity of Sanok), in complete calm, the northern horizon was suddenly illuminated up to a height of 40° (up to the middle of a cloudless sky) by a sheet-lightning-like flare similar to a Bengal Fire (refer to the same *Journal*). Amid this sea of fire, one significantly saw dark-red contours in the shape of a half sickle, the broader end of which was toward the bottom, and the other end of which appeared conical, running upward somewhat along an arc to a rounded-off point. The entire flight of the beautiful meteor did not last 5 seconds, while its luminous intensity decreased as it fell. The most surprising thing was that not the slightest noise of a detonation could be heard." At Biala on the boundary of Silesia (*ibid.*, p. 122), Mr. J. Strzygowski saw "an elliptical fireball, scattering fire like a rocket, fly in the

northeast direction. The nucleus of this ball was clearly visible and probably preserved an elliptical form throughout the flight. To the eye, it seemed to have a length of 1½ feet and a breadth of 1 foot. The sparks flew from it as if one were to throw into the air a bundle of ignited straw."

BOHEMIA; MORAVIA; ARCHDUCHY OF AUSTRIA; HUNGARY

The *Austrian Journal for Meteorology,* earlier referred to, contains the following statement in regard to observations made in the more distant parts of Austria: "At Kaadin (Bohemia, Saazer region), a dazzling red streak of light spread over the whole sky from northeast to southwest. The phenomenon lasted 4 to 5 seconds and was like lightning." Only the illumination could have reached into the southwest; the meteor itself appeared entirely in the east-northeast, from northern Bohemia. At Brünn, the phenomenon resembled a sea of fire and lasted several seconds. From Stockerau the following is reported (according to the detailed communication of Professor Eberhard Fugger in *Poggendorf Annalen,* **133**, p. 351): "At 6:50 in the evening, a blue fireball, which seemed to come from the Moon, appeared; it moved away from the Moon toward the southeast and grew ever larger and more luminous during its flight, spreading a blue light over the surroundings. The ball gradually disappeared behind the mountains on the right bank of the Danube after it had become smaller again, and, after its complete disappearance, one heard a brief thunder-like crackling. At the moment when the fireball seemed biggest, it appeared to move not much higher above the Earth's surface than about twice the height of the church tower. The phenomenon lasted 15 seconds in all." There flew above the city of Skalitz, in Hungary, also, "a large fiery meteor with a thunder-like crackling." Likewise, a peasant at Wieselburg is reported to "have seen the phenomenon flying above his head with a railroad-like rattling of his residence." At Ofen, Mr. Franz Schenk observed the fireball at 6:55, Ofen time. "It appeared in the north, low on the horizon, traversed the entire sky, and disappeared in the south about 10° above the horizon. Its path was approximately: Corona Borealis, β Ursae Minoris, between α and β Aurigae, to Orion. The color was deep red at appearance, then bluish-green, and later white. The observer did not notice a train; he estimated the size at half the Moon's diameter. The entire duration of the soundless phenomenon was 5 to 6 seconds." Differing from these and the preceding observations is the report from Losonz, made by Registered Physician Seydl, that trustworthy persons saw a large luminous fireball for one minute, moving from the west toward the south. Further, a Pester journal from Sünnyög, in *Beregher Comitat,* reports: "On the 30th, between 7:00 and 8:00 in the evening, 2 meteors rose upward from the southwest horizon for 25 seconds; they alternately shone and faded and then they moved northward together with extraordinary velocity, after which they disappeared with a great crash. In the same night a snow 5 inches deep fell. When, in the morning, the villagers drove their cattle to drink, the latter eagerly licked up the snow. Everyone was surprised, and, upon investigation, found that the snow was of a strongly salty taste. The people then collected the snow in vessels, and, when boiled and evaporated, each bottle of water gave ¼ pound of salt." Excluding this latter singular report, which is really not connected with the meteor, almost all these southern observations appear to contradict those from the north in regard to the course and direction of the meteor and are not compatible with them on essential points, for, at all these places, the meteor could have appeared only in the north. The inadmissibility of a southwest direction from

Kaadin in Bohemia has already been mentioned. At Stockerau, the ball *supposedly* moved from the Moon (which was in the southwest), toward the southeast, and *supposedly* disappeared behind the mountains on the right bank of the Danube. At Skalitz and Wieselburg, people *supposedly* saw the meteor above them. At Ofen, it *supposedly* coursed from north to south, with a definite statement of the constellations passed through; however, Corona Borealis, which was included among these, rests on a certainly erroneous identification, since it had not yet risen! Likewise, at Losonz, the motion is said to have been from west to south. Finally, at Sünnyög, the motion of 2 meteors was reputedly from the southwest northward. Not less inexplicable is the thunder-like crackling announced from 4 different places, altho nothing was heard of it in much closer Galicia. It will have to remain undecided until further investigation whether all this [evidence] is to be explained simply by inexactness of the observations, confusion of the widely spread luminosity with the meteor itself, odd noises, and the like, or whether, in actuality, 2 meteors, according to the explicit statement from Sünnyög, gave occasion to these contradictory reports.

EAST PRUSSIA; WEST PRUSSIA; POSEN

A valuable compilation of different reports about the meteor, especially from Prussia and Posen, was given by Mr. G. v. Boguslawski in Heis's *Weekly for Astronomy and Meteorology*, Nos. 12 and 15, from which I extract the following:

At Tilsit, the fireball was seen in the southeastern sky at a small height above the roofs of houses, as large as the Sun, and with a fiery cone directed upward. The appearance lasted only a moment; the observer thinks that the fireball had a fixed place in the sky. Other observers likewise saw it at a small altitude in the southern sky. In actuality, at Tilsit, the first outburst occurred at a height of 23°, and the meteor then dropped rather steeply, apparently in the line from northwest to southeast; however, the entire phenomenon did not show itself in the southeastern sky, but in the south-southwest.

From Insterburg, Dr. Pincus reports, in the *Insterburg Newspaper*, that, in the company of Teacher Guttmann from Hauschen, he observed the fireball between Insterburg and Georgenburg. "In the clear, moonlit sky there suddenly appeared, almost in the zenith [?], a star of the size and splendor of a white fireball; it first moved from northwest to southeast with a comet-like train extending farther and farther behind it. With continued flight, the fireball attained ever greater size and splendor, so that it soon became as large as the Sun, while the eye could not bear its brightness; the comet-like train changed into an extremely splendid cone of light, 20° long and of an alternate blue and green color. The region was lighted for some seconds as if by brilliant lightning, so that objects at a great distance became visible more clearly than by day. About 25° above the horizon the luster was suddenly extinguished, while the ball appeared to be dispersed into numerous glowing, dark-red fragments; the whole phenomenon lasted about 15 seconds." The statements of height are too great in this observation.

At Gumbinnen, the meteor appeared in the south; when it burst, a thunder-like explosion was perceived (distance 28 miles).

At Königsberg, the meteor was seen toward the southeast in a clear sky; "at first it was like a bright, red star suddenly flaring up; while it moved from northwest to southeast (dropping from a height of 45½° to one of 20°) it enlarged itself up to the size of the Sun and disappeared behind the houses after

2½ seconds; the light was an intense yellowish-green, the train slightly curved, like [that of] the great comet of 1858." If the height of 45½° depends upon an exact measure, then the meteor would have been noticed here at a height of almost 40 miles, as at Ragendorf in Hungary. "The fireball was seen also at Samlande and at Masovien; there a meteor is called *kolbuk*, and in the Polish Overland, *kobolt*." At Lyck (distance 18 miles), as at Gumbinnen, the detonation was heard. The reports from Hohenstein and Lötzen contain inaccuracies in regard to the direction.

At Friedrichshof near Ortelsburg, the fireball was seen moving toward Orion; its brightness was equal to that of the Sun; it gradually descended and later became eye-shaped and was dispersed into countless fiery sparks very close to the Earth. Very soon thereafter, a long-lasting, majestic clap of thunder rolled through the forest from the region of disappearance (distance 16 miles). According to the calculation, the meteor must have flared up at height 43° and azimuth 30° and then have been extinguished at height 25° and azimuth 344° (S.S.E.), not far from β Orionis.

The fireball was seen also at many places in West Prussia—among others, at Marienburg and Rosenburg. The accurate observation from Danzig has already been communicated in detail.

At Posen, the fireball was seen as a light moving from southwest to northeast. At Bromberg, the direction was the same and the duration was about 6 seconds; the light was greenish-white and so intense that streets and public places were illuminated as if in full daylight. At Schroda, travelers saw the fireball almost overhead (an estimate doubtless much too high). From Opalinica, it is announced that in the west, where the setting crescent Moon neared the horizon, a fireball rose with a long fiery train; it moved slowly and soundlessly like a rocket toward the east and disappeared at the horizon after 3 minutes. (The fireball could have been first seen only in the east-southeast, but moved in a line such that it had come apparently from the Moon.) A report from Meseritz describes the meteor as a column of fire 3 feet broad and 8 feet high that appeared to detach itself from the Moon, which a short time previous had been surrounded by a halo iridescent with all the colors of the rainbow. The column of fire, from which dropped single luminous fragments as large as a fist, moved toward the east, where it disappeared below the horizon. It diffused daylight and endured from ½ to 1 minute.

WERNIGERODE; FRANKFURT A. O.

The meteor was visible even at Wernigerode on the Harz, altho this place is 90 miles away. The observer there says: "Near 6:45 in the evening I stood at a place located somewhat above the valley and had a clear view to the east, limited, of course, by darkness. Suddenly the castle at Wernigerode, which lay about 20 minutes distant to the east of me and which thus far had been wrapped in night along with the mountains lying beyond it, became lighted up for approximately 4 seconds so brightly with a yellowish, reddish glow that I could perceive clearly the outlines of the castle and the windows in it. As seen from my station, this illumination arose from an apparently rather extended fiery mass, which, describing an arc of a circle, moved from northwest to southeast, not as a solid body, but as a luminous layer of clouds on the distant horizon, suspended in cosmic space." Between Frankfurt a. O. and Mullrose, the luminous phenomenon was seen in a completely clouded sky by Teacher Kogge from Fürsten-

walde. He writes: "The sky was very overcast and thickly covered by snow-clouds. It was so cloudy that the moonlight had almost no effect at all and only the white snowflakes lighted my way. Then there originated a light, very weak at first, but increasing to a maximum with great rapidity. This illumination was bluish in the beginning but then was like a very strong electric light. I quickly removed my hat in order to be able to look around better; but the sky was of uniform brightness and the light quickly decreased again. The entire phenomenon endured about 5 seconds. How vivid the light was is shown by the fact that the lamp of the toll-house, which was burning right in front of me, was hardly noticeable and that my coachman, who was driving high on his seat, said, after some minutes, 'I have been so frightened that I am still trembling!'" These 2 observations, [made] from distances of 90 and 50 miles and in a sky clouded in part, supply an especially significant demonstration of the great luminous intensity of the meteor.

[*To be continued*]

VARIABLE STARS

Variable Star Notes from the American Association of Variable Star Observers

By LEON CAMPBELL, Recorder

The 1942 A. A. V. S. O. Fall Meeting: In spite of the difficulties of travel, the A.A.V.S.O. Annual Meeting, held at Harvard on October 9 and 10, was a success, considered from the standpoint of attendance and interest displayed at all the sessions.

Forty members and guests were present, the one from Allentown, Pennsylvania, Mr. Howard Manlin, having traveled the greatest distance. Portland, Maine, New Haven and Norwalk, Connecticut, were also well represented, as well as Smith and Mount Holyoke Colleges.

The meeting on Friday evening was, as usual, held in conjunction with the Bond Astronomical Club, with the Amateur Telescope Makers of Boston also in attendance. At this session Lt.-Comdr. E. S. Rademacher gave us a very interesting talk on "Astronomy and the Evolution of the Mind of Man." Although the A.A.V.S.O. had been favored with a somewhat similar talk in Poughkeepsie in the spring of 1941, they were pleased to listen to Dr. Rademacher again, and his address, delivered in the Observatory lecture hall, was enthusiastically received by a full house.

The Council of the A.A.V.S.O. met on Friday afternoon and elected six new members: A. R. Boone, Webster, New York; J. W. Duffie, Montreal, Canada; P. B. Korneke, Jr., Scotia, New York; Helen M. Pillans, Mount Holyoke, Massachusetts; A. Sondel, New York, New York; W. H. Yealy, Derry, Pennsylvania.

The Council also voted to present the sixth A.A.V.S.O. Merit Award to Mr. D. F. Brocchi, of Seattle, Washington, long connected with the Association both as an active observer, and, for the past eight years, as chairman of the Chart Committee. Holders of previous Merit Awards are L. C. Peltier, T. C. H. Bouton, W. T. Olcott, E. H. Jones, and D. B. Pickering.

The matter of meetings in 1943 was delegated to a committee of three, the

President, the Secretary, and the Recorder, to be studied and reported upon to the Council at a later date.

The regular business meeting of the Association began at 10 o'clock on Saturday morning with President Dirk Brouwer in the chair. Reports of officers and committees were read and accepted. The reports of Messrs. Hartmann and Webb of the Chart Committee showed considerable activity in that department, nearly 2700 charts having been delivered to members, besides six copies of the A.A.V.S.O. star atlas. The reports of the Auroral, Photographic, and Nova Search sections showed decided diminution in activity during the past year. The President appointed a new Photographic Committee, consisting of Miss Marjorie Williams, C. W. Elmer, and the Recorder, the last named as chairman. With not too much hope for increased activity along photographic lines in the year ahead, the Committee welcomes inquiries regarding selection of programs and how to carry them out. The report of the Occultation Committee, presented by Miss Farnsworth and augmented by Dr. Brouwer, indicated a fair amount of activity in that department. The Committee asks not only for more observations of the occultations themselves, but also the enlisment of more computers for the reduction of observations.

The Recorder's eleventh annual report listed the activities under his supervision during the past year. The Charles Y. McAteer Library is the richer by 179 new acquisitions, including 43 volumes and parts of volumes of current astronomical magazines and reprints, as well as seven books on astronomy. The three-inch telescope owned by the late Sigmund Proctor, of Detroit, Michigan, was presented as a memorial by his mother, Mrs. Helen F. Proctor.

Bi-Monthly Bulletins, Variable Comments, the 1942 Julian day calendar, Variable Star Notes, and the Harvard *Annals* containing the original observations of variable stars by the members, have all been issued regularly.

The Recorder reported the extent of his investigation on the long-period variables. Up to October 1, he had discussed 250 variable stars, mostly of Me spectral type, handling approximately 450,000 observations, or 100,000 ten-day means, in the compilation of the mean light and intensity curves and the determination of the dates of maxima and minima.

Although the number of observations contributed during the year was lower than in recent years, as was to be expected, a creditable total was attained, namely 33,090, as against 37,443 for the previous year. Ninety observers took part in the work of keeping track of the variables, with Cyrus F. Fernald, of Wilton, Maine, heading the list with a total of 4,206 estimates. Next in order were Messrs. Holt of Tucson, Arizona, de Kock of South Africa, Cilley of Lewisburg, West Virginia, and Peltier of Delphos, Ohio, all of whom made more than 2100 observations each. Mrs. Kearons and Messrs. Hartmann, Jones, and Chandra attained the 1100-1800 class. The foreign observers numbered five from Canada, three from South Africa, two each from India, Australia, and Mexico, and one from Argentina.

In the 31 years since the formation of the Association, more than 880,000 observations have been accumulated on approximately 600 variable stars, including not only many long-period variables, but also numerous novae and especially erratic and irregular variables.

The annual election resulted in the choice of Messrs. Cilley, Kimball, Topham, and Erro as Council members for two years. The newly elected Council

then reelected the present officers for the ensuing year: President, Dirk Brouwer; First Vice-president, Roy A. Seely; Second Vice-president, Edward A. Halbach; Secretary, David W. Rosebrugh; Treasurer, Percy W. Witherell; Recorder, Leon Campbell.

Papers presented included "S Doradus," by S. Gaposchkin; "A Tiny Telescope," by D. W. Rosebrugh; "S Muscae," by A. W. J. Cousins; "Solar Problems for the Amateur," by D. H. Menzel; "Some Observations about an Observation," by J. J. Neale; "Nova Cygni 1942," by Joseph Ashbrook; "Kodachromes of an Amateur Observatory," by H. M. Harris.

The paper by Dr. Gaposchkin dealt with his nearly 1000 observations from Harvard plates of the long-perplexing variable, S Doradus, situated in the Large Magellanic Cloud, and discovered at Harvard in 1897. He finds that between 1889 and 1941 four minima, of about 0.6 magnitude range, occurred at intervals of about ten and thirty years, indicating a period of 40.2 years between primary minima. This result appears to indicate that S Doradus is a long-period eclipsing variable, in fact, the longest period now known. The secondary minimum is much displaced with respect to the primary. Each eclipse lasts for about three years, and all observed are alike in range and duration. The two components which form the system have radii of approximately 9.8 and 8.8 A.U., respectively, and are probably intrinsically the brightest known stars in the sky, with the system itself surpassing all known double star systems.

Mr. Rosebrugh described at some length his four-inch richest field telescope, and what he could do with it in observing variables down to the eleventh magnitude.

Mr. Cousins' paper on the Cepheid variable, S Muscae, treated his visual observations of 1924-26, 1936-37, and 1940-42, as well as the photographic observations compiled by L. V. Robinson. The visual light curve shows Zeta Geminorum characteristics, as would be expected, the primary maximum occurring almost exactly half way between the two minima. The differences found between visual and photographic observations may be partly accounted for by changes in color index. The period of 9.65942 days, found from the visual observations, appears to be nearly constant, with perhaps a slight tendency to increase during the past half century.

Dr. Menzel outlined possible programs for the interested amateur in the field of solar observations, not only in keeping daily records of sunspot areas and prominences but, if equipment would permit the use of a coronagraph, in keeping a close watch on the chromosphere of the sun.

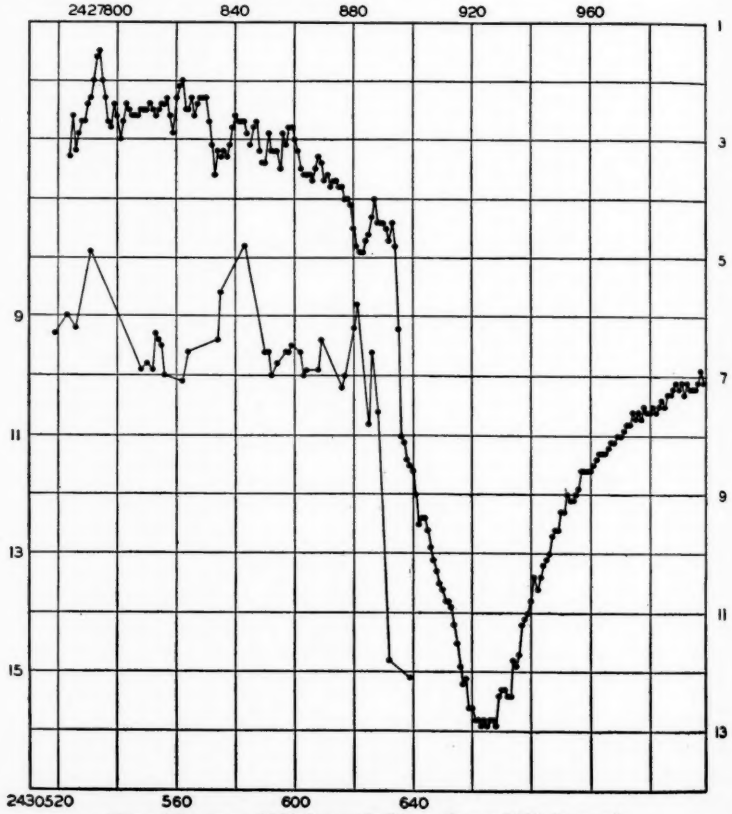
Mr. Neale reported on some advantages in observing lunar occultations of stars during lunar eclipses and during daylight hours. He referred in particular to the reappearance from occultation of Aldebaran on September 2 last.

Mr. H. M. Harris of South Portland, Maine, showed some Kodachromes of his observatory and the various steps in connection with the erection of the same.

Mr. Ashbrook reported on the work of Mrs. Ashbrook and Mrs. Nail of the Harvard Observatory staff in connection with the observations of Nova Cygni 1942, recently discovered by Dr. Zwicky at Palomar. This nova has proved to be of exceeding interest. The star was observed on at least sixty Harvard plates, the earliest date on which the image was seen being June 8, 1942. (This recalls to mind the appearance of Nova Aquilae on June 8, 1918, and the solar eclipse of that same day.)

For three months Nova Cygni oscillated in brightness around maximum, between magnitudes 7.8 and 10.8, with the brightest observations on June 20 and August 11. About September 25 the nova apparently began to fade away very rapidly, because early in October reports began to come in that the nova could not be seen in ordinary telescopes nor on plates of moderately long exposure. On September 29, Harvard plates recorded the nova as of magnitude 14.8.

If we compare the photographic light curve so far obtained for this nova with the visual light curve of Nova Herculis 1934, we at once see a remarkable resemblance in form, both at maximum and on the descent to minimum. Both novae remained at maximum approximately 100 days, with large oscillations in brightness, and their decreases in light were sudden and exceedingly rapid. If Nova Cygni is to behave as did Nova Herculis, we may expect it to decrease in brightness to between magnitudes 18 and 19, and then to recover its light to magnitude 12 or 13. By December 1, Nova Cygni should be well on the increase again, about magnitude 14, if it mimics Nova Herculis.



NOVA HERCULIS 1934 (UPPER), NOVA CYGNI 1942 (LOWER).

In the figure are shown the light curves of Nova Herculis 1934 (upper) and Nova Cygni 1942 (lower). The later nova may have been rising to its initial maximum on June 8, 1942, and perhaps the star began its sudden increase in brightness a day or two earlier.

Now that we have Nova Aurigae 1891, Nova Herculis 1934, Nova Cygni 1942, and possibly a few others, it begins to look as if we here have another distinct type of nova, not the recurring type, as for RS Ophiuchi, but one in which the star after maximum suffers a sudden slump in its normal activity and then recovers itself only to wend its way slowly again to extinction.

Following a noon recess, the out-of-town members were entertained at luncheon at the Gateway restaurant as the guests of Dr. and Mrs. Shapley.

With the completion of the A.A.V.S.O. program at three o'clock, the Observatory put on a colloquium which consisted of a symposium on variable stars. Mr. Campbell spoke on the correlations found for the Me long-period variables. Mrs. Gaposchkin reviewed the work done on the RV Tauri stars by herself and Miss Brenton, and Dr. Kopal reviewed the history and perplexities of Algol.

Tea was served in the lounge after the adjournment of the colloquium. In the evening a very informal dinner was held at the Harvard Faculty Club, attended by 35 members and guests.

The R Coronae Borealis Variables: In 1938 three of the four well-observed R Coronae Borealis-type variables were below normal maximum magnitude; SU Tauri, which was faint in August, and R Coronae Borealis and RY Sagittarii, both faint in October. (See these notes for February, 1939.) Again in 1942 the same condition prevails. SU Tauri decreased rapidly in April and May to below the thirteenth magnitude, and has been faint ever since. RY Sagittarii began to decrease in brightness late in August, but halted for a month or so at the eighth magnitude. It may again be on a further decrease in brightness at the present time. More recently, early in October, R Coronae Borealis began to fade away, and by the eleventh, had decreased to the eighth magnitude with no sign of a halt. S Apodis, the fourth variable of this type, has remained near normal maximum since 1935, with no indication of a drop in magnitude up to August 23.

All four of the R Coronae-type variables were below normal brightness in 1935. With three of these stars again below normal maximum brightness, after an interval of four years, there is presented a remarkable coincidence, to say the least.

Observations received during September, 1942:

Name	Var.	Obs.	Name	Var.	Obs.
Ashbrook	1	4	Hartmann	139	144
Blunck	18	24	Heckencamp	3	11
Bouton	20	25	Hiett	12	22
Buckstaff	8	15	Holt	130	216
Carpenter	6	6	Howarth	9	13
Cilley	21	47	Huffer	15	19
Cousins	44	97	Jones	64	157
Duffie	20	26	Kearons	83	158
Erdman	13	19	Kelly	10	11
Fernald	238	677	Koons	53	117
Ford	20	20	Labrecque	20	20
Garneau	25	45	Manlin	28	28
Giese	2	2	Mason	16	17
Griffin	41	41	Meek	23	115
Halbach	12	16	Moore	12	12

Observations received during September, 1942, continued:

Nadeau	37	57	Taulman	13	13
Parks	27	57	Treadwell	3	3
Peltier	148	243	Vohman	12	16
Reeves	2	3	Webb	24	24
Rosebrugh	7	21	Weber	101	120
Schoenke	11	11	—		
Sill	23	26	42	Totals	2718

October 17, 1942.

Comet Notes

By G. VAN BIESBROECK

The month has been quiet for comets since none are known to be in reach of ordinary telescopes. It is true that on September 26 observatories received a delayed and garbled announcement of the discovery of a comet by Oterma at Turku (Finland) but a subsequent communication established the fast that this object was none other than PERIODIC COMET SCHWASSMANN-WACHMANN No. 1, which has been mentioned frequently in these notes and for which an ephemeris was given last month. This remote object continues to behave in a manner different from all other comets in that its brightness undergoes large fluctuations while from the distance relations no appreciable change would be expected over several months. At the beginning of October it had dropped from its earlier magnitude of 12 to magnitude 16 and, when last recorded here on October 10 with the 82-inch reflector, it had gone down further to a tiny nebulosity hardly as bright as a 17th magnitude star. However, from previous experience we may expect a rapid increase in brightness any moment.

No other comets are under observation at this time. PERIODIC COMET FORBES is now too faint to be followed further. PERIODIC COMET WOLF will apparently go by unobserved at this return.

W. J. McDonald Observatory, Fort Davis, Texas, 1942 October 11.

Communications and Comments

Under this heading we shall publish from time to time such material as does not properly fall under any of the established headings of this journal. Here, too, may be found, when occasion arises, articles which the editor may not be willing to give sanction to but which, nevertheless, may be provocative of thought along new lines.

The Great Stellar Cycle

ABSTRACT

The paper traces the life history of a star, based upon astronomical observations and physical theories which are the common property of science. It is found to be possible that stellar evolution is cyclical in nature so that a star, passing from the giant stage through the "main sequence" development into the dwarf phase, will eventually return to the giant stage when a repetition of events will take place, though not necessarily with all of the same material. It is shown how energy which is lost as radiated heat again participates in atom formation, and

how, although the Second Law of Thermodynamics holds for every locality, a dead level of temperature throughout the galaxy may never be reached.

Russell's spectrum-luminosity diagram suggests that a star begins its life history as a giant or supergiant, a comparatively cool star, and ends it as a dwarf which may warm up to a white-hot maximum and then cool off to a cold sun. If stars begin life as giants when they appear on the stage of cosmic events, they must be characterized by an average density so small that the air at sea level is millions of times more dense, according to the scientific opinion of the day. This ponderous but exceedingly tenuous mass of gas is usually in rotation, produced perhaps by collision and coalescence with another mass, the two having been drawn together by gravitational attraction.

Increasing temperature results as the mass contracts, the potential energy which the gas particles possess by reason of their separation is transformed into kinetic energy which, according to the kinetic theory of gas, is heat. The giant star thus diminishes in size and increases in density and temperature until its external temperature becomes so great that the loss of energy due to radiation overbalances the gain of internal heat resulting perhaps from contraction as already noted, and from the transformation of mass into energy. The luminosity of stars of large mass is usually greater than it is for those of smaller mass. The supergiants thus become very brilliant as they contract, finally becoming stars of perhaps spectral class O, when a balance is reached between the generation of heat and its radiation. Giants of smaller mass and greater density find themselves in spectral classes B, A, and perhaps F when the balance is reached. This finishes the first phase of the star's history.

At this critical point when the radiation and supply of heat become equal, the star begins to cool as it contracts, passing again through the spectral classes but now in the reverse order, from O to M down the "main sequence." The interior pressure increases as this second phase progresses, rising to extreme values which, together with the enormous thermal activity and perhaps the diminishing size of their quarters, cause the atoms in the central region of the star to break up. Eddington pictures this tumult as follows: "Dishevelled atoms tear along at 50 miles a second with only a few tatters left of their elaborate cloaks of electrons torn from them in the scrimmage. The lost electrons are speeding a hundred times faster to find new resting places. Look out! there is nearly a collision as an electron approaches an atomic nucleus; but putting on speed it sweeps around it in a sharp curve. A thousand narrow shaves happen to the electron in 10^{-10} of a second . . . Then . . . the electron is fairly caught and attached to the atom, and its career of freedom is at an end. But only for an instant . . . And what is the result of all this bustle? Very little . . . The atoms and the electrons for all their hurry never get anywhere; they only change places." One accomplishment of this activity is the radiation of energy as the cooling star approaches the M class along the main sequence.

Then as contraction proceeds and radiation diminishes (because the increasing density of the star makes it more difficult for the ether waves to extricate themselves from it), a condition is finally reached in which the trapped heat causes the internal temperature of the star to rise to a lofty maximum. This is the third phase here noted of the star's history, characterized by contraction with increasing temperature. The star now warms up from perhaps spectral class M

and becomes a white-hot dwarf as it approaches class O. Its radiating machinery has become so demolished during the time of increasing heat during the dwarf period that radiation is at a minimum. But after internal activity has reached its maximum because further transformations into energy cannot keep pace even with the low radiation that has existed, the star begins to cool, and its radiating apparatus undergoes an overhauling and much-needed repair. As cooling continues the star finally passes into apparent oblivion as a cold dense mass swinging under the resultant attraction of the galaxy, as it whiles away its fourth phase of history.

While hurtling through space, this cold star may sometimes collide with another, or be brought into gravitational association with it, the presence of other stars so changing the orbits that the two stars settle down to revolving about their common center of gravity, and perhaps together revolving about a third star, the three forming a group similar to the sun-earth-moon system. G. H. Darwin has traced the possible future history of the earth and moon as modified by the tides on these bodies (see "Tides" by Darwin). He finds that an exchange of energy takes place between them, causing the moon's orbit to enlarge to a stable maximum when the periods of revolution and rotation will be equal. But, because of the presence of the sun, the equilibrium of the system becomes disturbed and energy is fed back to the earth as the moon gradually spirals down toward it until their periods of revolution and rotation again become equal. This equality of periods however may not always take place in a multiple star system in which the stars were not formed from one another. In this case when the orbits diminish to a minimum, rubbing contact may take place as the stars circle about one another.

The heat resulting from this frictional activity would cause a sudden expansion of the material, perhaps to the extent that the two stars would vaporize and again return to gas. Thus, whether by collision or by spiraling toward one another to make rubbing contact, the two stars would experience such a violent increase in temperature that a flare-up would occur like that witnessed in "new stars." The outbursts of "new stars" may perhaps be the signals that such stellar collisions or other contacts have occurred. During the first outrush of gaseous material there is a terrific increase in temperature because of the enormous generation of heat, but gravity finally arrests the outward plunge even for those particles having speeds approaching the velocity of escape. The final expansion due to the outward momentum of the particles is adiabatic in nature, and with dwindling heat supply causes the great mass of exceedingly tenuous gas to drop in temperature, and to approach the spectrum of class M giant stars. It seems probable that relatively very few stars would reach the cold sun stage before the catastrophe occurred which would return them to the giant stage, and so a shorter stellar cycle would take place. Perhaps that which regulates the length of a stellar cycle has some relation to Russell's "giant stuff" and "dwarf stuff."

An alternate stellar activity which might possibly take place when the star has reached the stage of a white-hot dwarf is the following: Because of the enormously high temperature and density of the white dwarf, other things also contributing in perhaps some unknown way, such as the mass of the star whether large or small, a condition will evenually develop in the star which leads to sudden loss of equilibrium due to some reaction, and the heat thereby generated pushes the particles of gas violently outward. This separation of the stripped atoms from their crowded condition is the trigger action which brings about a sudden inrush

of the electrons which were hovering around the star, attracted to its vicinity by the unbalanced electric charge of the stripped atoms. The potential energy of this enormous cloud of inrushing electrons is thus suddenly transformed into kinetic energy as they seek out normal levels in the atoms of the star. This change to kinetic energy produces a terrific release of heat causing a tremendous expansion of the gas, resulting in the extreme and sudden brilliancy of a new star. The continued outward rush of the material because of inertia produces adiabatic cooling, and the brilliancy of the star declines as well as the radiation pressure within it. As the radiation pressure decreases, the attraction of gravitation within the

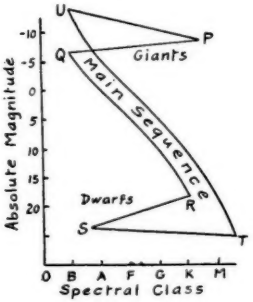


FIGURE 1

mass comes into the ascendency and the star contracts with the result that the temperature increases. The heat thus produced causes a return of the outward pressure, and the mass of gas again expands. The attraction of gravity and the pressure of radiation thus alternately come into the ascendency producing in the mass of gaseous material alternate swelling and shrinking with corresponding fluctuations in the brilliancy of the star. This oscillatory phenomenon has been observed repeatedly in new stars during the gradual decline in brilliancy after the flare-up. If the oscillations continue indefinitely perhaps a Cepheid variable would result. To show this stellar activity on the diagram, the closing line of the great stellar cycle would omit the detour to point T and go more or less directly from S to U, and then oscillate on the line between U and P, this last line representing the pulsations of the Cepheid variable or the oscillations which subside in new star fashion. A study of the next paragraph will make the graph of these stellar activities clearer.

Referring to the diagram, the great stellar cycle begins at P (say) with the star as a giant of rather low temperature and very low density. Contracting, it increases in temperature and density, passing to Q at the upper end of the "main sequence." Still contracting and increasing in density, the star now drops in temperature as it passes down the "main sequence" to R, where radiation has diminished until it balances internal heat production. The star now heats up to an extreme maximum at S at which point heat production falls to a low level, causing the star to pass out as a cold sun at T. At T, or at any previous time during its history, the star participates in a collision or picks up a companion which, circling it in an orbit, finally grazes its surface. In either case a sudden and violent increase in temperature occurs, vaporizing the material, and the star returns to U as a gaseous mass. Adiabatic cooling returns the star to P, and the great

stellar cycle is completed.

Throughout a stellar cycle, a star would exchange radiant energy with its neighbors in the galaxy, the exchange perhaps even reaching to other galaxies. Attraction governs within the galaxy, except for some molecular distances, although the galaxies themselves seem to be separating, as deduced from the red shift of their spectral lines. The radiant energy which has gone out in immense quantities as heat from giant and "main sequence" stars into interstellar and perhaps into intergalactic space may be continually swept up by these colossal masses of cold tenuous gas while they move swiftly about during the astronomical ages, and this radiated material may thus again participate in atom formation. The completion of the stellar cycle would thus include the return of energy which seemed to have been lost as heat through radiation into space, and which is now returned for a repetition of events. If radiation cannot pass beyond the limits of a galaxy, the eventual return of radiated heat to the stars of the galaxy would be expected.

While throughout the galaxy during such stellar cycles, heat everywhere passes from the warmer to the cooler region, as it does on the earth in accordance with the Second Law of Thermodynamics, yet, in the giant and dwarf phases, cooler bodies warm up because of the transformation of mass into energy and of energy from the potential to the kinetic form. While the Second Law seems to hold at all times for every locality in the galaxy yet the galaxy as a whole, because of such energy transformations, does not seem to be approaching maximum entropy, that is, to a dead level of temperature throughout.

With Eddington we again say: "What is the result of it all?" Is it just a cyclical operation on a universal scale, and nothing more? Is there no purpose in it all? When things happen, man naturally looks for a purpose. As the mind of man which is capable of investigating this great material universe is generally regarded as the world's crowning glory, so it is natural to regard the stellar cycle as merely the stage setting for a revelation of vital significance planned by the Great Designer. How important then such a lesson must be for man for, though his material contribution is insignificant to the vanishing point, yet his presence may be of such real significance in other ways as to be one of the foundation stones in the structure of this universe of wonders!

HAWLEY O. TAYLOR.

Sunspot Activity During the 1937 Cycle

The sources for the following notes are (1) articles from current astronomical publications and (2) telescopic observations.

The sunspot cycle, which began in 1933, reached its peak in 1937, and is now almost over, was the greatest since that of 1870. It was a cycle of remarkable interest including the most intense magnetic storm ever recorded (March 24, 1940), the two highest solar prominences ever measured (September 17, 1937, and March 20, 1938), and the most brilliant aurora borealis seen in fifty years in England (January 24, 1938). Besides, it also had the 5th largest sunspot group (October 4, 1937) since measurements were begun in 1872. It was during this cycle that the outstanding motion pictures and studies of solar prominences were developed by Dr. Robert McMath of the McMath-Hulbert Observatory.

The cycles of 1870 and 1937 are the two highest both in numbers and areas of sunspots since 1832 which is as far back as records of areas go. The average

total sunspot areas of the peaks of cycles is shown in the following table, in which areas are expressed in millionths of the sun's visible hemisphere:

Year	Area	Year	Area
1836	1331	1893	1464
1847	1021	1905	1191
1859	1404	1917	1537
1870	2761	1928	1390
1883	1155	1937	2073

RELATIVE HEIGHTS OF THE SUNSPOT-CYCLES

1938 was the peak year for solar prominences while 1937 was the peak year for magnetic activity. In frequency of spots, July, 1938, was the peak of the cycle but according to spot-areas, July, 1937, came highest. In number of large sunspot groups (exceeding 1000 millionths in area) 1937 was highest, having had 16 while 1938 had 14 and 1936 had 7. The outstanding magnetic storms occurred on April 16, 1939, March 24, 1940, and September 18, 1941. The majority of great sunspots occur at the peak of the cycle and afterwards, and the largest prominences also occur the same way so this is probably due to lagging culmination of activity. In general it is noticed that solar phenomena have more rapid growth than decline.

The decline of this cycle has remained at a very high level of activity. It is remarkable that the giant solar spot of September 16, 1941, appeared 4 years after the maximum was reached, as it is nearly the largest one of the cycle. It is also interesting that 1939, the second year after the maximum, has an average sunspot area of 1800 which is higher than that reached by the very peaks of any of the other cycles except that of 1870.

Activity through the cycle occurred as follows: 1933 was the minimum year of sunspots with the minimum occurring in August. In 1934 sunspots gradually increased with the first large group of the cycle passing the central meridian on April 21. 1935 was a year of steadily increasing sunspots with some large groups in February and June and a giant spot-group crossing the sun on December 2. Solar activity suddenly increased very much by the end of the year. 1936 was a year of considerable activity. From the beginning of the year until August sunspots declined then increased greatly again until the end of the year when three larged naked-eye spots were visible on the sun at the same time on December 1. Activity continued to rise sharply into the secondary peak period of January, 1937.

1937 and 1938 contained, respectively, 537 and 547 groups. In the beginning of 1938 there was the giant solar group (3000 millionths) of January 18 which caused the aurora of January 24. This aurora was the brightest display seen in England for 50 years. It was seen farther south than any previous aurora, being seen in Italy, and in the widest area since 1709. Its red color caused it to be mistaken for fires at many places. A prominent pair of round spots was on the sun in the later part of March. On March 20 the highest prominence in history (detached eruptive flame) rose to a height of 1,000,000 miles. Spots became very numerous in April and May, declined in June, then reached their greatest numbers in July. They died down in August and September, then three giant sunspots appeared in November and December.

In 1939 the gradual decline of solar activity continued at about the same slow rate since 1937. This year was largely uneventful with the main activity occurring in May and September. On July 8 there was a fine display of three

large groups on the disk. Two of them were 1000 and 1700 millionths in area. The two great groups of the year showed within a few days of each other on August 31 and September 10 during the high September peak. They were both complex penumbral structures with many nuclei.

1940 was a year of accelerating decline whose high point was in August. However, it had 2 interesting phenomena. The solar group of January 5 while huge was also prominent because of the pair of big black nuclei dominating it and was widely visible during foggy weather. With smoked glass it appeared more impressive than bigger groups which were less concentrated. The display of September 16, 1941, was disappointing to the naked eye for this reason. The spot of March 24 caused the most intense magnetic storm ever recorded, doing severe damage to electrical communications. A rare tornado prominence was recorded on September 14.

1941 was generally low in activity following the decline, except for an active period in June-July and the isolated big sunspot group of September 16. By July 26 a beautiful collection of two big groups was on the sun. The giant sunspot of September 16 was one of the most complicated seen with inummerable nuclei enmeshed in a network of penumbra. It was accompanied by a severe magnetic storm and a fine aurora borealis which was seen in Chicago as displays of greenish and silvery rayed curtains filling the sky. Also a boreal crown and a hooked curtain was seen. It was visible even in Florida.

The year of 1942 so far is uneventful in activity, except for a big display of March 1 with a curved cyclonic stream of nuclei.

LESTER SUSSMAN.

1411 S. Sawyer Ave., Chicago, Illinois, June, 1942.

General Notes

American Astronomical Society Meeting.—The Council of the American Astronomical Society has decided that the winter meeting is to be held as usual this year. The meeting will take place at the Dearborn Observatory, Northwestern University, Evanston, Illinois, December 28, 29, and 30.

The Rittenhouse Astronomical Society held its regular monthly meeting on Friday, October 9, 1942, in the Lecture Hall of The Franklin Institute. Dr. Louis C. Green, assistant professor of astronomy, Haverford College, spoke on the topic "Interstellar Absorption Lines." A dinner in honor of Dr. Green preceded the meeting.

Meteor Crater and Petrified Forests

Proceeding on the bizarre hypothesis that the meteor which blasted out Meteor Crater might also have felled the trees in the various petrified forests of northeastern Arizona, Richard L. Feldman and a family party, on summer tour in the Southwest in 1941, reported interesting results. Data for the Holbrook, Cameron, and Ganado areas seemed to show a common orientation of the tree trunks of each of the respective areas, when trees obviously displaced were ignored. Three lines of direction were thus obtained, which, prolonged, form a triangle having Meteor Crater in the midst. The bisectors of the angles of this triangle meet at a point 17 miles, N 67° E from Meteor Crater, or 10 miles N x W from Winslow. A close

association of the petrified trees with volcanic material and the "bad lands" or Painted Desert, was noted. Sunset Crater, an extinct volcano, lies not far to the west of Meteor Crater.

"Fantastic," said one geologist. "Faulty premise;" said another; "the petrified trees fell and altered to stone in Triassic time, whereas the big meteor must have fallen fairly recently."

"But the meteor dug into Carboniferous strata," replies the vacationist-turned-explorer, "whereas the Triassic period followed the Carboniferous."

And there the story hangs.

Book Reviews

Astronomy for Night Watchers, by J. B. Sidgwick. (Messrs. Faber and Faber, Limited, 24 Russell Square, London. 5/-net.)

The title quite clearly indicates the character and purpose of the book. It is further described by the publisher as follows:

"At the present time thousands of men and women in the civil defense services are for the first time in their lives spending long night hours out of doors. On the nights that are not enlivened by 'incidents' the study of the heavens, whether with the naked eye or with binoculars, is a fascinating pastime which not only opens up new realms to the human mind, but which will end once and for all the tedium of the night watches.

"The author describes the changing appearances of the night sky and gives a summary of modern knowledge regarding the planets, moon, stars, comets and meteors, star clusters, and nebulae. A special section tells you how to find your way at night by means of the stars. The second half of the book is devoted to descriptions of the individual constellations: how to find them, and the many objects of special interest for observers with binoculars. These are illustrated by forty-seven star maps. In addition, there is a lunar map and four seasonal maps showing the constellations of winter, spring, summer, and autumn."

Norton's Star Atlas and Telescopic Handbook, (Eighth Edition) by Arthur P. Norton and J. Gall Inglis. (Gall and Inglis, 13 Henrietta Street, London.)

The eight edition of this well-known work among star atlases was issued in June, 1942. Throughout the series of editions, which began in 1910, there has been general enlargement and improvement. The present edition contains the following new features:

1. A list with full particulars of the brightest and the nearest stars.
2. A table of Boyer and Lacaille Letters and Flamsteed Numbers.
3. Hints on the adjustments of Newtonian and Equatorial telescopes.
4. A section-drawing and description of a small Equatorial head.
5. A revised and enlarged index of contents.

As a result this edition is a veritable compendium of astronomical facts and helpful suggestions. The excellent star maps are especially worthy of mention and commendation. This latest edition is up-to-date in the matters which change with time. A copy of it, therefore, would be a very useful and helpful volume in any library in which astronomical questions come up for consideration.

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